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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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The Journal *of* Parapsychology

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The Journal *of*
Parapsychology

VOLUME 25 DECEMBER, 1961 NUMBER 4

A QUARTER-CENTURY OF THE JOURNAL OF PARAPSYCHOLOGY: A BRIEF REVIEW

By J. B. RHINE

AS THE ONLY survivor of the founding editors of the *Journal of Parapsychology,* I have been asked by the present staff to look back with an appraising eye over the twenty-five years that have been completed with this issue. It is gratifying to be requested to scan in retrospect the years so centrally important in the development of parapsychology. The greatest satisfaction, of course, comes from the fact that the *JP* has, in many ways, made progress during this period and can be recognized by all as an important "institution" in the parapsychological domain.

The commentary to follow is a sort of review; but it is not a history, either of the *JP* or of the field of parapsychology during these twenty-five years. At the same time, whatever interest the review may have will largely be the result of the fact that since 1937 the *JP* has itself been an essential part of the history of the field. Accordingly, the actual details and events of the editorial program over the years are important in themselves and would naturally belong in any proper history. But in the present sketch I shall compromise by adding at the end a table of events in the development of the *JP* showing the changes in editorial personnel, the types of consulting arrangement, and the introduction and timing of various new features.

The viewpoint from which the following remarks are made is perhaps obvious. There is ground enough for a certain pride on the part of all those many individuals who more or less identify their interest with the *JP*. But a more appropriate feeling is one of

gratitude for the helping hands that have contributed to its establishment and its period of existence. There come first to mind the strong loyalties, the generous financial contributions—more especially needed during its early years, for now it can at least pay its own printing bills—and the countless and often unscheduled hours of editorial work of which only those who have shared in it are fully aware.

The *JP* has been fortunate to have had the backing of Duke University through the Duke University Press and the Parapsychology Laboratory. The latter has provided, during most of these twenty-five years, the staff which has carried the main burden of editorial work. It is unlikely that many will ever realize the basic role this fostering of the *JP* has played in the development of the science of parapsychology. Yet all can agree that the *JP* had to have some sustaining associations during its trying years of growth.

But the best service such a review can render is to draw attention to past efforts to improve the periodical, to point up such traditions as have developed, and to try to make available the general lessons of experience for whatever they may be worth. These reminders may be helpful in decisions to be made in the future, if the *JP* continues to be, as it has been, not a static, but a growing affair.

A first point to mention, one perhaps not widely realized today even among parapsychological readers, is that the *JP* owes much to the fact that the late Professor McDougall was one of its founding editors. Dr. McDougall had already assisted in the launching of the *British Journal of Psychology* as well as the *Journal of Personality* (then known as *Character and Personality*). When, therefore, he wrote the editorial introduction to the first issue of the *JP*—an article that well deserves reading or re-reading a quarter of a century later—it was with a background of experience in related areas. Making an initial statement of policy, after an explanation of the reasons for adopting the term "parapsychology" as part of the title, he went on to say: ". . . it is our intention to admit to this journal only contributions that properly fall within the narrower sphere implied by its title; that is to say, reports of experimental studies in the stricter sense and discussion of methods and interpretations of such work." This policy, which is still adhered to, was thus "nominated in the charter."

Another of the cardinal points set forth in the McDougall editorial dealt with the reasons for the establishment of the *JP*: "In the absence of some such journal as this there would be much inevitable delay in publication of practically all contributions of the kind which we invite. All the regular psychological journals seem to have difficulty in finding sufficient space for suitable matter offered to them."

Attention may significantly be drawn also to the declaration of principles in this historic statement: "We shall do our best to maintain a high critical standard, to guard against errors of all kinds, and to protect our readers against lack of good faith on the part of any contributor. . . . We are acutely aware of the heavy responsibility towards the public which we are undertaking. It is largely for this reason that we have abstained from asking the co-operation of others whose names, listed as an editorial board, might have adorned and given distinction to our cover. . . . We invite their frankest and severest criticism, believing that such criticism is much needed in this branch, as in all other branches, of psychology." Such was the level of responsibility on which the *JP* was launched.

It is true, names have since been added to the editorial page in addition to those of the editors. However, such additions have been made for utilitarian rather than honorary purposes. First, there came the addition of the Board of Review, set up after the annual meeting of the American Psychological Association in September, 1938, to provide regular criticism. This Board consisted of a nine-man staff of psychologists critically oriented toward parapsychology who were willing to review articles before publication, with the understanding that their criticisms would be published along with the articles. This arrangement, however, lasted approximately only two years and was then abandoned partly because of many resignations and partly because of the slowing-down of publication due to delays in the reviewing. But even if only for a short period, the experiment of having a Board of Review represents the extremes to which the editors were willing to go in the spirit of the initial editorial policy to "invite the frankest and severest criticism." During its time, some of the more important articles in the *JP* went through the Board of Review and did so relatively unscathed.

The next departure to be mentioned was the addition of an Ad-

visory Editorial Board for the years 1948 through 1956. This Board was added with a view to widening the range of creative thinking and thereby making the *JP* "more broad and international in its orientation." Four men, well-known leaders in parapsychological thought (Dr. Hornell Hart, Dr. Gardner Murphy, Dr. H. H. Price, and Dr. R. H. Thouless) were asked to serve on this Board. It was thought by the editors that with such a group of consultants, more diversified undertakings might be considered. But through no fault of the Board members, this plan did not continue to achieve the desired effect and, at the time of a change of editorship, the idea was abandoned.

A third decision to add names to the editorial page was one that has proved wise, perhaps beyond any other major change in the history of the *JP*. This was the invitation to a competent authority to serve as editorial adviser on statistical material under consideration for publication. In 1942, Dr. J. A. Greenwood became the first statistical editor and in 1945, Dr. T. N. E. Greville came onto the staff to share that responsibility. From that time up to the present, these two mathematicians have generously and most faithfully supervised the statistical editing of the *JP*, and nothing involving statistical matters has been published without first having their critical appraisal. This is probably a unique editorial policy in scientific publication. It has been a service of the greatest importance to the maintenance of the high standards to which Professor McDougall's editorial committed the *Journal*. It is doubtful if many scholars who take up parapsychology in its later stages will ever fully realize the historic significance of this vigilant service during the controversial years. At any rate, to have statistical editors has become one of the "traditions" of the *JP*.

Not all the changes which were introduced, however, involved new names on the title page. Some of the other changes have been natural developments, such as the inclusion of book reviews. These were excluded by express policy from the earlier volumes; but beginning in 1942, books of distinctly parapsychological character and of special significance to the field have been reviewed.

Another change was the decision in 1946 to publish separately material which had in earlier volumes been included under News and Notes. Thus, the small, four-to-six-page quarterly publication

known as the *Parapsychology Bulletin* became the first offspring of the *JP*. It was to be sent to subscribers to the *JP* between numbers. Edited at first by Dorothy H. Pope of the *JP* staff, the *Bulletin* appeared regularly four times a year from 1946 until 1956. Then publication was suspended. But a little over a year later, it became a publication of the Parapsychology Laboratory at Duke, with no specific editorial commitment and no longer officially connected with the *JP*. This enabled it to serve its purpose effectively as a general news bulletin, with responsibility to be assumed by the Laboratory staff as distinguished from the editorial staff of the *JP*.

In 1958, the section of the *JP* known as Parapsychological Abstracts was introduced. The aim of this section was to cover all parapsychological publications—foreign as well as domestic—that would be of value to students and that were not reported in full in the *JP*. This innovation is one of the well-conceived and now firmly established practices.

Another of the important developments in the *JP* history was the affiliation of the *JP* in 1958 with the Parapsychological Association. This connection called for the introduction of the section on Association news, programs, and abstracts of convention papers, and of course the *JP* goes out to all Association members. Naturally it was of great advantage to the Parapsychological Association to have affiliation with a scientific periodical that was already established and had twenty years of back volumes; a journal, too, that is self-maintaining and even available to the membership (at a discount which contributes to the support of the P.A.). But the advantages are mutual. The P.A. affiliation has added definitely to the status of the *JP*, and the mutual benefit should increase with the advancement of parapsychology.

As of any scientific periodical, the real backbone of the *JP*, after the research it reports, is in its editorial policy; and to this less manifest but more determinative factor we must look for effective appraisal. In addition to the strong commitment on policy set forth in the McDougall editorial in March, 1937, other clarifications have been made from time to time; most expressly, for example, when the Duke editors resumed responsibility, in 1942, after the three-year shift to New York; and again, in March, 1950, in a long

editorial entitled "Publication Policy," the thought developed on these problems was outlined.

Editorial attention has at least three main areas on which to focus: first is the policy with regard to reader clientele; second, that toward the authors and the standards of acceptance; and third, most important of all, the policy as regards the field as a whole, with respect to what the *JP* aims at accomplishing. Most questions of policy will fall under one of these three; for example, the important question of policy with regard to the P.A. is part of the third.

The first problem confronting the editors with regard to the reading public was that of finding a clientele. In 1937 serious students of parapsychology were too few to support a journal or justify one. The subject was, however, of sufficient interest to intelligent laymen—that is to say, educated people from various professional and vocational fields—to make it seem likely that if articles could be made sufficiently readable they would reach a fair section of this nonparapsychological public. Beginning, then, with the publication of a glossary with each number, and with a short abstract in nontechnical language preceding each article, the editors aimed at the broader objective not only of making their publication useful to the technical student, but also interesting to a wider readership. The *JP* was criticized from both sides, but more often the clamor was louder for a more readable, understandable periodical. The passage of time naturally increased the number of readers capable of following the more technical articles and worked in favor of the group preferring the *JP* to be a thoroughly "scientific" one with no concessions to the popular reader. The affiliation with the P.A. served only to strengthen this already well-established trend.

Regarding their relations to contributors, editors in general would probably agree that their most painful moments are those which occasionally come in their dealings with the "rejected" author. Editors in better established fields, in which standards become cut and dried and acceptance a more routine matter, will, perhaps, know little of the difficulties of editing a periodical in a pioneer field. Even if adequate formulation of standards of acceptability are worked out well in advance—and in what field has this ever been

done?—the job of the editor in applying these to the individual case is difficult indeed, even with the help of a competent advisory staff (and pioneering editors seldom have enough help at the stage when it is most needed). Add to all this the fact that people who, as authors, pioneer in a new field are likely to be strong-minded, enthusiastic, and not easily reconciled when it comes to recommendation of drastic changes in presentation or another outlet of publication.

An occasional author has taken his frustration personally and has not been fair and reasonable in response to decisions of the editors. While in the nature of the case there can be no proper court of appeal from editorial decision, there are many petty annoyances to which the editors can be subjected by the author who takes rejection as a bitter personal afront. Against such action the editor himself has no higher court of appeal either. But if the detachment of retirement from the editorial scene allows me to make a general judgment, it would be that the *JP* has survived this trial by ordeal, and this is salve enough for the editors' wounds.

Standards of acceptance have changed as time has gone on. They have tended to be more refined as the research has progressed. During early years, almost any article that furnished evidence of ESP under conditions comparable to those of general psychology were admissible to the *JP* pages. Discussions of methods, more especially statistical methods, found ready acceptance if they bore upon the growing needs of the young science. Over the years, as controversy about psi waxed more vigorous, the increasingly complex precautionary measures in the researches were reflected in the articles accepted for publication. While today a certain number of articles still appear which are identified as exploratory, in the conduct of which the test procedure has been something less than conclusive, such reports are now required to make some distinctive contribution in order to warrant acceptance for publication. Thus editorial standards have progressively improved without becoming oppressively exacting. Thus on the matter of standards the heritage of the quarter-century is good.

With regard to the acceptance of critical articles, the standards of the early years have been upheld. It was stated at the outset that criticism was invited, and, indeed, over the years it has ever been solicited. When serious criticism affecting work presented in the

JP was made elsewhere, its author was invited to submit a report to the *JP*. So vigorously has this policy been pursued that he has been an elusive critic, indeed, whose remarks have not appeared in the pages of the *JP*—always, it need hardly be added, with an appropriate reply where necessary.

There have been limits to the consideration in the *JP* of criticisms of the research, as no one will deny there should be. For example, some readers have wondered that there has not been more of an editorial disposition to pursue nonscholarly authors of "fringe" criticism who have carped at the psi research because to do so had popular appeal. Editorial policy has, however, drawn the line at giving serious attention to the critic who was himself unqualified or not well enough informed to weigh and argue his criticism.

The third responsibility of the editorial policy is to the field itself. Those who undertake, in whatever way, to maintain a scientific periodical, must have in perspective what they hope to accomplish. In the *JP* the founding editors definitely hoped to find enough qualified material for publication to justify subscription and support, to make the periodical a proper and convenient *center* for the reports of experimental projects, their methods, discussion of results, interpretation, and the like. Some workers in the field urged, rather, the advisability of scattering research reports around through existing psychological journals and even into still wider areas of circulation. Still others had certain affiliations with psychical research societies and felt an obligation to present their reports to the journals of those societies. Other reasons were offered for a dispersing rather than concentrating policy of publication.

Gradually, over the years, a policy of issuing, whenever possible, the first full report of a research in the *JP*, has acquired a measure of success, perhaps as much as can be expected from a profession of such individualists as a pioneer field attracts. As a result, today, with the addition of the section on Parapsychological Abstracts, the isolated worker in the field who has access only to the *JP* will miss comparatively little of the significant research developments.

In the guidance and formation of editorial policy over the years, there has been a great need for constant vigilance, for readiness to meet the changing problems and opportunities even while holding steadfastly to certain tried and true standards and objectives. In

many respects the *JP* today is not the same as it was twenty-five years ago, and its flexibility will surely allow it to be different again in other ways when another twenty-five years have passed. Such adaptability is important to any form of growth. By this steady adaptation to the varying demands being made upon it, the *JP* has survived. It has brought from its first quarter-century the promise of a good beginning on a second. If any challenge to the field of parapsychology exists today—and a great many certainly do—one of these is to see that the standards of the *JP* are upheld, its progressive quality maintained by vigilant adaptability, and its responsibility to the reader, to the author, and to the field in general tirelessly sustained.

Chronology of Events in the History of the Journal of Parapsychology

1937 Publication of the first number of the *Journal*, a quarterly, issued at the Parapsychology Laboratory of Duke University under the aegis of the Duke University Press.

Editorial staff (1937-1938): William McDougall and J. B. Rhine, Editors; C. E. Stuart, Assistant Editor.

1939 Establishment of the Board of Review composed of Dr. Saul B. Sells, Chairman, Lillian Dick, J. J. Gibson, E. R. Hilgard, J. L. Kennedy, Irving Lorge, Louis Long, John Volkmann, and R. R. Willoughby. The Board functioned through the year 1941.

Editorial office shifted to New York City.

Editorial staff (1939-1941): Gardner Murphy and Bernard F. Riess, Editors; Ernest Taves, Managing Editor.

1942 Return of the editorial office to Duke University.

Appointment of the first *Journal* statistical editor, J. A. Greenwood.

Addition of book reviews to *Journal* contents.

Editorial staff (1942): J. B. Rhine, C. E. Stuart, J. G. Pratt, Editors; J. A. Greenwood, Statistical Editor; Dorothy H. Pope, Managing Editor.

1943 Editorial staff (1943-1944): J. B. Rhine and J. G. Pratt, Editors; C. E. Stuart, Associate Editor; J. A. Greenwood, Statistical Editor; Dorothy H. Pope, Managing Editor.

1945 Addition to the staff of a second statistical editor, T. N. E. Greville.

Editorial staff (1945-1946): J. B. Rhine, J. G. Pratt, C. E. Stuart, Editors; Betty M. Humphrey, Assistant Editor; J. A. Greenwood

and T. N. E. Greville, Statistical Editors; Dorothy H. Pope, Managing Editor.

1946 Publication of the first number of the *Parapsychology Bulletin* as an adjunct to the *Journal*. Dorothy H. Pope, Editor.

1947 Editorial staff (1947-1951): J. B. Rhine, J. G. Pratt, and Betty M. Humphrey, Editors; J. A. Greenwood and T. N. E. Greville, Statistical Editors; Dorothy H. Pope, Managing Editor.

1948 Appointment of Advisory Editorial Board composed of Hornell Hart, Gardner Murphy, H. H. Price, and R. H. Thouless. The Board served until the end of 1956.

1952 Editorial staff (1952-1955): J. B. Rhine and J. G. Pratt, Editors; Elizabeth McMahan, Assistant Editor; J. A. Greenwood and T. N. E. Greville, Statistical Editors; Dorothy H. Pope, Managing Editor.

1956 Discontinuance of the *Parapsychology Bulletin* with the 41st number (April, 1956).

Editorial staff (1956-1957): J. B. Rhine, J. G. Pratt, and L. E. Rhine, Editors; Dorothy H. Pope, Assistant Editor; J. A. Greenwood and T. N. E. Greville, Statistical Editors.

1957 Resumption of publication of *Parapsychology Bulletin* in August, 1957.

1958 Selection by the newly formed Parapsychological Association of the *Journal* as its official publication, and addition of a new section to the *Journal* to contain the Association announcements and the programs of the annual conventions.

Creation of a new section known as Parapsychological Abstracts to contain resumés of reports in other publications as well as reports of unpublished research.

1959 Editorial staff (1959-1960): J. G. Pratt, L. E. Rhine, and W. G. Roll, Editors; Dorothy H. Pope, Managing Editor; J. A. Greenwood and T. N. E. Greville, Statistical Editors.

1961 Editorial staff: J. G. Pratt and L. E. Rhine, Editors; Dorothy H. Pope, Managing Editor; J. A. Greenwood and T. N. E. Greville, Statistical Editors.

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A TWO-EXPERIMENTER INVESTIGATION OF TEACHER-PUPIL ATTITUDES AND CLAIRVOYANCE TEST RESULTS IN THE HIGH-SCHOOL CLASSROOM[1]

By MARK E. RILLING, CLARE PETTIJOHN, and JOHN Q. ADAMS

ABSTRACT: The purpose of this research was to determine whether the work of Anderson-White on the effect of student-teacher attitudes on ESP could be repeated with significant results. The subjects were high-school students and there were two experimenters, each conducting tests in a separate high school. The primary expectation—that in the over-all results there would be a significant difference between the ESP scores of the students who liked the teacher and those who had a negative attitude toward the teacher—were not borne out. The results of one of the experimenters were significant, but the comparison of the total scores gave statistically insignificant results. The scoring was examined also from the point of view of the teacher's attitude toward the subject. The difference based on the total scores was significant, but in the opposite direction from that found by A-W: here the subjects who were preferred by the teachers scored negatively as a group and those who were not preferred, positively. The ESP results were also examined with regard to mutual student-teacher attitudes. When the attitudes were alike (that is, when teachers and students were positive toward each other or were negative toward each other) a comparison of the scores showed the difference to be at the chance level. But when the mutual attitudes were opposite, the difference was significant, which, again, was a reversal of the A-W results. The authors take these contradictory findings to indicate that the variables in such experiments are more complex than previous researches have indicated.

One of the purposes of the experiment was to assess the influence of the experimenter on the scoring. There were differences in the results of the two experimenters, but they were not consistent and it was concluded that the effects of such individual differences need further investigation.

This research was conducted while the three authors were undergraduates at Oberlin College. Mr. Rilling, the senior author, is now a graduate student in psychology at the University of Maryland.—Ed.

INTRODUCTION

A REVIEW by Van de Castle in 1959 reported the findings of 17 ESP experiments which were carried out in the high-school or elementary classroom (15). Two main types of experimental design were used in these experiments. The first type was a GESP test developed by the Dutch investigator J. G. van Busschbach. In

[1] This work was supported by a grant from the Parapsychology Laboratory of Duke University. The authors would like to express their thanks to the teachers and students who participated in these experiments.

a series of four experiments with first-, second-, fifth-, and sixth-grade pupils (**10, 11, 12, 13, 14**), van Busschbach found that significant results were obtained when the teacher was the agent or "sender." He obtained chance results when the agent was another adult or a pupil in the class in the elementary school studies and also when the studies were extended to include seventh- and eighth-grade and secondary school pupils. The van Busschbach experiments have been successfully replicated by Louwerens with kindergarten pupils (**7**), and similar results have been obtained by Vasse and Vasse (**16**), who tested first-grade pupils with clairvoyance test procedures.

The second experimental design was developed by the American investigators Anderson and White (who will be referred to as A-W). This design is an improvement over the earlier van Busschbach procedure in two respects. First, the type of ESP test used is clairvoyance, which allows easier control of the possibility of sensory cues than does a GESP test. Second, the results are analyzed on the basis of independently measured teacher-pupil attitudes. This correlational method may be used to obtain evidence for ESP as well as to study the relationship between teacher-pupil attitudes and ESP.

In two experiments with high-school pupils (**2, 3**) and a third experiment with fifth- and sixth-grade elementary pupils (**1**), Anderson and White found the following generally consistent results: 1. Students who had a positive attitude toward the teacher tended to have scores above chance, and students who had a negative attitude toward the teacher tended to score below chance. *2.* Similarly, students to whom the teacher had a positive attitude tended to score above chance, and students to whom the teacher had a negative attitude tended to score below chance. 3. The largest deviations were observed for the groups in which both pupil and teacher were positive or both pupil and teacher were negative.

There have been a number of experiments attempting to replicate the A-W studies with either elementary- or high-school students. Using fourth- and fifth-grade pupils as subjects, Hall (**6**) combined the GESP test of van Busschbach with the A-W technique of using teacher-pupil questionnaires. His results were not significant. Eisenbud (**4**) tested fifth- and sixth-graders a few days before

Christmas vacation with chance results. Rilling, Adams, and Fisher (9) tested a class of fifth-graders over a period of several months during which each pupil was given 500 trials instead of the usual 100 given by A-W. In a second experiment by Rilling and Adams (8), fourth- and fifth-grade pupils were given 100 trials apiece. The results for the attitude of the student toward the teacher, for this experiment and the first 100 trials of the first experiment combined, were at a suggestive level of significance, with $P = .047$. The most careful attempt to replicate the A-W classroom experiments is that of Goldstone and Deguisne (5). Using fifth- and sixth-grade pupils, Goldstone obtained chance results in all of his analyses. Thus, of the five experiments using the A-W procedure or modifications of it in the elementary grades, only one was marginally successful.

The A-W experiment with high-school students has been replicated twice: once by A-W and once, at least partially, by Deguisne (5). Using high-school students, Deguisne obtained significant results for the attitude of the student toward the teacher and insignificant results for the attitude of the teacher toward the student. Thus the A-W procedure has given more reliable results when used with high-school students.

The purpose of this experiment was to determine whether the A-W procedure could again be repeated with significant results with a group of high-school students as subjects. Since Deguisne had obtained significant results and since Goldstone had not, the authors decided to divide the work between two experimenters to determine whether any differences in results might be due to the experimenters rather than to the grades to which the experiment was given. M.R. conducted the experiment with three Oberlin, Ohio, high-school teachers and their pupils, and C.P. conducted the experiment with three Towson, Maryland, high-school teachers and their pupils. The Minnesota Teacher Attitude Inventory, a measure of a teacher's rapport with his pupils, was given to each teacher to determine whether rapport had an influence on the teacher's ability to administer the ESP test.

The experiments using the A-W procedure indicate that significant results are obtained when the test is given to high-school students and the data are analyzed according to the attitude of the

student toward the teacher. The primary analysis of the present experiment was, therefore, to be based on the attitude of the student toward the teacher. It was predicted that with a group of high-school students as subjects, there would be a significant relationship between the attitude of the student toward the teacher and the clairvoyance test score of the student. In the event that this hypothesis was not confirmed, it was decided also to analyze the results in this same way separately for the subjects of each experimenter.

Method

The method of this experiment involved slight modifications from that of A-W (2, 3). C.P., who graduated from Towson High School in Towson, Maryland, obtained the co-operation of three teachers who agreed to use one class apiece to participate in the experiment. C.P. knew one of the teachers and it was at the suggestion of this teacher that the experiment was initiated. She contributed two of her classes, so that altogether four classes at Towson participated in the project. Three teachers from Oberlin High School in Oberlin, Ohio, who were recommended by the principal, were asked to participate in the project by M.R., who was not acquainted with these teachers. One of the Oberlin teachers tested two of his classes so that four classes at Oberlin were tested. In both high schools the teachers were asked to select for the experiment their best classes on the basis of intelligence and academic performance.

Several days before the ESP test was scheduled, each student was given the A-W questionnaire designed to measure the attitude of the high-school student toward his teacher. Each teacher received a set of instructions based on those worked out by A-W (2, pp. 153-55). The experimenters modified them to fit each high school. The teachers were encouraged to tell, not read, the instructions to their classes, and to fit the test into their course material as much as possible. They were also urged to motivate their students to do their best on the test in order to please the teacher personally.

The following is a summary of the method for preparing, administering, and checking the ESP test which is given in full by A-W (3, pp. 143-46). At the beginning of the test, each student received a large manila envelope which contained a standard ESP

record sheet on which were written four runs of ESP symbols, 25 symbols to the run. These target sheets were made up from random number tables by C.P., so that each subject received a test with a different order of symbols. The sheets were numbered and a carbon copy of each sheet was kept by C.P. Two blank ESP record sheets, between which a carbon had been inserted, were stapled to the envelopes, thereby sealing the targets inside. The two records of the subjects' guesses and the two records of the targets enabled the experimenters to check the results independently.

Each student was asked to write on the outer record sheet his guesses of the order of the symbols inside the envelope. Students were encouraged to make their guesses in their own way and at their own rate of speed.

Finally, each teacher was asked to complete the Pupil Evaluation Questionnaire which is designed to measure the attitude of the teacher toward the student. The teachers were to answer *Yes* or *No* to the following question: "If you could form your ideal group for this class, would you include this student?" Each teacher also completed the Minnesota Teacher Attitude Inventory.

The A-W questionnaires were used to prepare the data presented in this paper. For the attitude of the teacher toward the student, students who were ranked *Yes* by the teacher formed the positive group, and students who were ranked *No* by the teacher formed the negative group. For the attitude of the student toward the teacher, students whose answers on the student questionnaire gave a high rating to the teacher (scores of 10-9 and 8-7) formed the positive group and students who rated the teacher low (scores of 6-0) formed the negative group.

The ESP tests were independently scored for each subject, using the master and the carbon sheets. The scores were triple-checked by C.P. The same method was used to score the questionnaires.

Results

Two different methods for the statistical evaluation of the results of these experiments have been used. The first is the total deviation from chance for a group of subjects and the statistic used is the critical ratio where N is the total number of trials. The second method dichotomizes the ESP results for each subject into the

Table 1

ESP Results for the Three Levels of Student's Attitude Toward Teacher

Experimenter	Rating of Teacher by Student	Number of Subjects	Number of Runs	Average Run Score	Deviation
C.P. (Towson H.S.)	10-9 (Positive)	40	160	5.08	+13
	8-7 (Positive)	43	172	5.31	+53
	Total Positive Ratings	83	332	5.20	+66
	6-0 (Negative)	38	152	4.70	−45
M.R. (Oberlin H.S.)	10-9 (Positive)	38	152	4.91	−14
	8-7 (Positive)	24	96	4.93	− 7
	Total Positive Ratings	62	248	4.92	−21
	6-0 (Negative)	33	132	5.04	+ 5
Total Subjects	10-9 (Positive)	78	312	5.00	− 1
	8-7 (Positive)	67	268	5.17	+46
	Total Positive Ratings	145	580	5.08	+45
	6-0 (Negative)	71	284	4.86	−40

Total Subjects: CR_d (Pos. Attitude—Neg. Attitude) = 1.52; P = .13.
C.P.'s Subjects: CR_d (Pos. Attitude—Neg. Attitude) = 2.55; P = .011. P(times a correction factor of 3) = .033.
M.R.'s Subjects: CR_d (Pos. Attitude—Neg. Attitude) = .56; P = .58.

categories "above chance" and "at or below chance" and the attitudes into "positive" and "negative," and the statistic used is the chi-square where N is the total number of subjects. In testing hypotheses concerning teacher-pupil attitudes, it is necessary to use the individual subject as the unit of analysis so that the results of each subject are given equal weight. The data as analyzed by the CR method are relevant to the occurrence of ESP but not to the effect of attitude.

The data were collected by six teachers from eight classes containing, altogether, a total of 216 subjects who were given a total of 864 runs or 21,600 trials. The data are analyzed in Table I according to the hypotheses. For the first hypothesis—that there would be a significant difference between the ESP scores of the students who had a positive attitude toward the teacher and the ESP scores

Table 2

DISTRIBUTION OF SUBJECTS SEPARATED ON BASIS OF ESP SCORE LEVEL IN RELATION TO STUDENT'S ATTITUDE TOWARD TEACHER

	Student Attitude Positive	Student Attitude Negative	Total
Students with ESP Scores Above M. C. E.	75	24	99
Students with ESP Scores at or Below M. C. E.	70	47	117
Total	145	71	216

$\chi^2 = 6.17$ (1 d.f.)
$P = .013$

of the students who had a negative attitude toward the teacher, as measured by the A-W questionnaire[2]—the critical ratio of the difference is 1.52, which is not significant.

For the second hypothesis—that there would be a significant difference between the two attitudes for one experimenter's data—the results are also presented in Table 1. For M.R.'s 95 subjects, the CR of the difference is .56, which is not significant. For C.P.'s 121 subjects the CR of the difference is 2.55, which is significant with $P = .011$. This probability is one of three computed in accordance with the hypotheses, and it must therefore be multiplied by 3 to correct for the number of analyses. The corrected P is .033, which is not significant.

The results of all subjects for the chi-square analysis of the student ESP score in relation to the student's attitude toward the teacher are presented in Table 2. The chi-square equals 6.17 (1 d.f.) with $P = .013$, which is marginally significant.

The extent to which the ESP results are related to the teacher's attitude toward the student is given in Table 3. The 70 subjects of C.P. toward whom the teacher is positive have an average run score of 4.87. The 51 subjects toward whom the teacher is negative

[2] The mean of the student questionnaire is 7.11 with a standard deviation of 2.27. The split-half reliability of the A-W student questionnaire is .85, which is significant with $P < .01$. These results indicate that this questionnaire is a reliable measure of student attitude.

Table 3

ESP Results as Related to Teacher's Attitude Toward Student

Experimenter	Teacher's Attitude Toward Student	Number of Subjects	Number of Runs	Average Run Score	Deviation
C.P.	Positive	70	280	4.87	−37
(Towson H.S.)	Negative	51	204	5.28	+58
M.R.	Positive	74	296	4.85	−45
(Oberlin H.S.)	Negative	21	84	5.35	+29
Total Subjects	Positive	144	576	4.86	−82
	Negative	72	288	5.30	+87

Total Subjects: CR_d (Pos. Attitude—Neg. Attitude) = 2.91; P = .0036.
C.P.'s Subjects: CR_d (Pos. Attitude—Neg. Attitude) = .69; P = .49.
M.R.'s Subjects: CR_d (Pos. Attitude—Neg. Attitude) = 1.90; P = .056.

have an average run score of 5.28. (M.C.E. for the average run score is 5.00 since there are 25 trials to the run with $p = 1/5$.) The CR of the difference between the positive and negative subjects of C.P. is .69, which is not significant. The 74 subjects of M.R. toward whom the teacher was positive have an average run score of 4.85 and the 21 subjects toward whom the teacher was negative have an average run score of 5.35. The CR of the difference for the subjects of M.R. is 1.90 with $P = .056$, which is not significant. For all subjects, the 144 students toward whom the teacher was positive have an average run score of 4.86 and the 72 subjects toward whom the teacher was negative have an average run score of 5.30. The CR of the difference between all the positive and negative subjects is 2.91 with $P = .0036$, which is significant.

Whether student ESP score level is related to the attitude of the teacher toward the student is tested in Table 4. The chi-square (1 d.f.) equals 3.37, with $P = .066$, which is not significant.

The interactions between the attitude of the student toward the teacher and the attitude of the teacher toward the student for all of the subjects are presented in Table 5. The 102 cases involving mutually positive attitudes have an average run score of 4.96 and the 29 mutually negative cases have an average run score of 5.22. The CR of the difference between the mutually positive cases and the mutually negative cases is 1.03 which is not significant. When

Table 4

DISTRIBUTION OF SUBJECTS SEPARATED ON BASIS OF ESP SCORE LEVEL IN RELATION TO TEACHER'S ATTITUDE TOWARD STUDENT

	Teacher Attitude Positive	Teacher Attitude Negative	Total
Students with ESP Scores Above M. C. E.	59	39	98
Students with ESP Scores at or Below M. C. E.	85	33	118
Total	144	72	216

$\chi^2 = 3.37$ (1 d.f.)
P = .066

Table 5

ESP RESULTS AS RELATED TO ATTITUDE OF TEACHER AND STUDENT TOWARD EACH OTHER

	TEACHER'S ATTITUDE POSITIVE					TEACHER'S ATTITUDE NEGATIVE				
Rating of Teacher by Student	No. of Subj.	No. of Runs	Av. Run Score	CR	P	No. of Subj.	No. of Runs	Av. Run Score	CR	P
10-9 (Positive)..	61	244	4.94	—	—	17	68	5.21	—	—
8-7 (Positive)..	41	164	4.99	—	—	26	104	5.46	2.35	.019
Total Positive Ratings..	102	408	4.96	—	—	43	172	5.36	2.36	.018
6-0 (Negative)..	42	168	4.61	2.51	.012	29	116	5.22	—	—

Same Attitudes: CR_d (Teacher Pos. and Student Pos.—Teacher Neg. and Student Neg.) = 1.03; P = .30
Opposite Attitudes: CR_d (Teacher Pos. and Student Neg.—Teacher Neg. and Student Pos.) = 3.45; P = .00056.

the student is negative and the teacher is positive, the 42 subjects in this category have an average run score of 4.61. This deviation is marginally significant, with a CR of 2.51 and P = .012. On the other hand, when the student is positive and the teacher is negative,

the 43 students in this category have an average run score of 5.36. This deviation is marginally significant, with a CR of 2.36 and P = .018. When the student and the teacher have opposite attitudes toward each other the CR of the difference is 3.45, with P = .00056.

Whether the difference in the direction of the scoring on the ESP test is related to the opposite attitudes of teacher and student toward each other is tested in Table 6. The chi-square equals 10.02 (1 d.f.) with P = .0016.

Table 6

DISTRIBUTION OF SUBJECTS SEPARATED ON BASIS OF STUDENT ESP SCORE LEVEL WHEN STUDENT AND TEACHER HAD OPPOSITE ATTITUDES TOWARD EACH OTHER

	Teacher Positive, Student Negative	Teacher Negative, Student Positive	Total
Students with ESP Scores Above M. C. E.	12	27	39
Students with ESP Scores Below M. C. E.	30	16	46
Total	42	43	85

$\chi^2 = 10.02$ (1 d.f.)
P = .0016

DISCUSSION

On the basis of the results, the hypothesis designated for primary consideration (the significant correlation of student attitude and ESP scoring) was not verified. Therefore this experiment failed to repeat the results reported in the two high-school experiments of A-W and the work of Deguisne. However, it is interesting to note that the data of Towson H. S., when analyzed separately, were marginally significant and that the chi-square analysis for all the subjects was marginally significant. These results reveal at least directional similarities with earlier work.

The results for all subjects showed a significant difference in the ESP scoring when the teacher's attitude was positive and when

it was negative, but the direction of the deviation was reversed from that reported by A-W. In this experiment the students who were rated as positive by the teacher tended as a group to score below chance and the students who were rated as negative by the teacher tended as a group to score above chance. It is difficult to explain this reversal, but it should caution other workers from concluding that the attitudes in question are always *positively* correlated with ESP scores. It may be that factors other than teacher-pupil attitudes, such as the way in which the test is presented to the students, influence the direction of the deviation. At least we are prevented from assuming that the teacher's attitude is responsible because of the fact that the chi-square for the student ESP score level in relation to the teacher's attitude toward the student is not significant.

One of the major purposes of this project was to begin to develop a method for assessing the influence of the experimenter on the results of classroom tests. C.P. had significant results (P of the difference = .011) for the attitude of the student toward the teacher and M.R. had chance results; but M.R. had suggestive results for the differences in attitude of the teacher toward the student and C.P. had chance results. It cannot be concluded that these differences were due to the experimenters alone. It is interesting to note that the M.T.A.I. scores for the Towson teachers tended to be higher than the M.T.A.I. scores for the Oberlin teachers. Further research will be necessary to determine the effects of individual differences among teachers and experimenters on classroom ESP tests. Previous acquaintance with the teachers may also be an important factor in successful classroom work.

In the first A-W experiment (**2**), students who were negative toward the teacher when the teacher was positive scored significantly below chance, but the score for this same group in the second A-W experiment was at chance. About the latter result A-W (**3**, p. 96) comment: "We can only say that this does not bear out the previous A-W results on this point and it may, therefore, be taken less seriously." However, in the experiments of Deguisne (**5**) this group scored below chance, with marginal significance. Furthermore, in the present experiment, when the student was positive toward the teacher and the teacher was negative toward the student, the group scored above chance, with marginal significance. The

total results of the comparison of mutual attitudes showed a significant difference between the students to whom the teacher had an *opposite* attitude, rather than a significant difference between students and teachers with like attitudes, as was reported in the two A-W experiments. These contradictory findings call for a reassessment of the concept that mutually positive attitudes produce positive ESP scores and that mutually negative attitudes produce negative ESP scores.

This experiment was not designed to test the repeatability of the A-W procedure as used for a criterion for the existence of ESP. Repeatability is an inadequate criterion for judging evidence because there are no criteria for deciding how closely a replication must resemble the original research nor how much similarity there must be among the results for the experiment to be a successful replication. The findings of this experiment suggest that the variables which influence the results are more complex than previous classroom studies have indicated. This should caution other workers who intend to use this method not to neglect psychological conditions; for example, the motivation requirements which must be satisfied in the classroom experiments just as in other types of psi research.

References

1. Anderson, Margaret. Clairvoyance and teacher-pupil attitudes in fifth and sixth grades. *J. Parapsychol.*, 1957, **21**, 1-11.
2. Anderson, Margaret, and White, Rhea. Teacher-pupil attitudes and clairvoyance test results. *J. Parapsychol.*, 1956, **20**, 141-57.
3. ———. A further investigation of teacher-pupil attitudes and clairvoyance test results. *J. Parapsychol.*, 1957, **21**, 81-97.
4. Eisenbud, J. Personal correspondence. January 5, 1959. Cited by R. L. Van de Castle in "A Review of ESP Tests Carried out in the Classroom." *Int. J. Parapsychol.*, 1959, **1**, 84-102.
5. Deguisne, A., Goldstone, G., and Crumbaugh, J. C. Two repetitions of the Anderson-White investigation of teacher-pupil attitudes and clairvoyance test results. *J. Parapsychol.*, 1959, **23**, 196-214.
6. Hall, J. A. GESP tests with fourth- and fifth-grade pupils. (Abstract of paper read at Parapsychological Association Convention, September, 1958.)
7. Louwerens, Nicolauda G. ESP experiments with nursery school children in the Netherlands. *J. Parapsychol.*, 1960, **24**, 75-93.

8. RILLING, M. E., AND ADAMS, J. Q. Teacher-pupil attitudes and clairvoyance test scores in the college and elementary classroom. *J. Parapsychol.*, 1959, **23**, 291. (Abstract of paper read at Parapsychological Association Convention, September, 1959.)
9. RILLING, M. E., ADAMS, J. Q., AND FISHER, J. R. An exploratory experiment on teacher-pupil attitudes and clairvoyance test results. (Unpublished manuscript.)
10. VAN BUSSCHBACH, J. G. An investigation of extrasensory perception in school children. *J. Parapsychol.*, 1953, **17**, 210-14.
11. ———. A further report on an investigation of ESP in school children. *J. Parapsychol.*, 1955, **19**, 73-81.
12. ———. An investigation of ESP between teacher and pupils in American schools. *J. Parapsychol.*, 1956, **20**, 71-80.
13. ———. An investigation of ESP between teacher and pupils in the first and second grades. *J. Parapsychol.*, 1959, **23**, 227-37.
14. ———. An investigation of ESP in first and second grades in American schools. *J. Parapsychol.*, 1961, **5**, 161-74.
15. VAN DE CASTLE, R. L. A review of ESP tests carried out in the classroom. *Int. J. Parapsychol.*, 1959, **1**, 84-102.
16. VASSE, CHRISTIANE, AND VASSE, P. ESP tests with French first-grade school children. *J. Parapsychol.*, 1958, **22**, 187-203.

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AN ESP TEST INVOLVING EMOTIONALLY TONED OBJECTS[1]

By JOHN FREEMAN[1]

ABSTRACT: This GESP experiment was designed to test the hypothesis that emotional attachment to a target facilitates ESP response to it. Each of 12 subjects furnished five objects toward which he had some emotional attachment. These were the targets for half the tests. ESP symbols were targets for the other half. The experimenter, acting as agent, was in a separate room from the subject. Keeping in step with signals from the subject, the agent placed his hand over the object or ESP symbol selected as the target for each call. The subject, in the other room, recorded his calls. Although the over-all difference in scoring between objects and symbols was in the direction anticipated (higher scoring for the objects than for the cards), it was not significant. However, a test of significance based on the differences between the two kinds of targets for the individual subjects gave a significant result ($P = .004$). Eleven of the 12 subjects scored in opposite directions for the two types of targets.

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MANY of the cases of spontaneous ESP which have been reported involve situations which have strong emotional content. This suggests that the ability to perceive a target through ESP might be facilitated if there were some emotional attachment to the target. Published reports of the few experiments done in this area seem somewhat contradictory, however. Fisk and West report that in a test with erotic symbols and ESP cards a significant deviation from chance was obtained which was due entirely to an excess of correct calls on the erotic symbols. They suggest that target symbols carrying strong emotional significance may facilitate ESP scoring with subjects who are otherwise unsuccessful (2). Skibinsky found that in a comparison of ESP targets and targets made up of given names of participants in the test (who were all closely related), the subjects showed a total negative deviation from chance expectation with the names and a total positive deviation with the symbols. While neither deviation was significant when taken alone, the CR

[1] This work was supported in part by a Ralph Drake Perry Fellowship in Parapsychology.

of the difference was significant. He suggests that the personal associations with the names caused the negative scoring (5). Upton Sinclair commented about his wife's tests with drawings to the effect that she could get the "feeling" or emotional content even when she did not get the exact target (4). Whately Carington, as part of his discussion on "Experiments on Paranormal Cognition of Drawings," said emotion may play an indirect role in the transmission process, but the general tone of his discussion shows that he really did not think the emotional elements to be at all important (1). In a test with French first-grade school children, P. Vasse and C. Vasse concluded that the kind of target was not important, but rather that it was the generally favorable psychological conditions that mattered (6).

With these views in mind, I designed the present experiment to see what difference, if any, would appear when a subject participated in an ESP test using emotionally toned objects as targets and the standard ESP cards as control targets. My expectation at the outset was that higher scores would be obtained with the emotionally toned objects. However, the experiment was exploratory, and I was interested in observing any trends in the results which seemed to indicate the occurrence of ESP and might throw some light upon the psychological factors influencing it.

The subjects were all Duke University students (with one exception, a University of North Carolina student) who came voluntarily to the Parapsychology Laboratory to serve as subjects in any of the current experiments. Every student who wanted to participate was allowed to do so. That is, there was no selection or classification of subjects on the basis of preliminary testing or interview.

Procedure

The first session with each subject was devoted to general conversation to get acquainted, to explain and build up interest in the experiment, and to schedule the first ESP test session. During this first visit to the laboratory each student was asked if he had five objects, toward which he felt some emotional attachment, that he could bring to the laboratory as a part of the experiment. It was explained that these objects would be used as targets in some of

the sessions, and the regular ESP cards would be used in the remaining sessions.

Two rooms were used in the experiment, with the subject in one room and the experimenter, acting as agent, in the other. The door between the rooms was kept closed during the run. The experimenter had a felt-covered table in front of him on which either the subject's objects were placed in alphabetical order or the five ESP symbols, depending on whether objects or symbols were to be the targets in that particular session.

Before each session another member of the laboratory staff prepared a list of numbers from the Rand Corporation's *A Million Random Digits* and then transposed the digits to letters representing the five objects or to ESP symbols, as required. This master target sheet was never seen by the subject. The agent copied the target run-by-run as the experiment developed. After a beginning signal was given by the subject, the agent would place his hand over the target appearing first in the random order. When the subject indicated that he had recorded whichever target he thought was then being covered, the experimenter placed his hand on the next target and the subject recorded his response for it. This procedure was followed for twenty-five trials, after which the experimenter would go into the room where the subject was and check the order of the targets against the subject's guesses. The experimenter recorded the score of the run on his record sheet.

Two sessions of five runs per session were carried out with each subject using the objects as targets, and two sessions of five runs per session using the ESP cards as targets. Half of the subjects started with objects and the others with symbols, and each subject did the two kinds of targets in regular alternation. The last two subjects joined the experiment late and they, therefore, completed only one session of five runs with the objects and one session of five runs with the cards. It had been decided at the outset that the experiment would be concluded by the Thanksgiving holidays, and these last two subjects could arrange only two sessions each.

Results

Ten subjects did a total of 10 runs each with the cards and 10 runs each with the objects, while two subjects did a total of five

runs each with the cards and five runs each with the objects. This gave a total of 110 runs with the cards and 110 runs with the objects. In each case the mean chance expectation would be 550, but the total hits for the cards was 529 (a negative deviation of −21) and the total hits for the objects was 570 (a positive deviation of +20). The CR of the difference is not significant, so the hypothesis that emotional attachment to the target facilitates ESP was not borne out. However, there were trends in the results which were so pronounced and consistent that they attracted my attention while the experiment was in progress. Even though I recognized that no final conclusion about them could be reached in this experiment, statistical evaluations were made as a guide to judgment and tentative interpretation.

Table 1

EVALUATION OF INDIVIDUAL SUBJECT DIFFERENCES IN SCORING ON OBJECTS AND ESP SYMBOLS AS TARGETS

Subject	Objects		ESP Cards		Diff.	CR_d	χ^2
	Runs	Dev.	Runs	Dev.			
R.A.	10	− 2	10	+ 5	7	.78	.61
R.B.	10	+15	10	−10	25	2.80	7.82
K.G.	10	− 6	10	+ 6	12	1.34	1.80
R.Ha.	10	+ 3	10	−11	14	1.57	2.45
R.He.	10	− 8	10	+ 4	12	1.34	1.80
J.H.	10	−10	10	+ 1	11	1.23	1.51
G.H.	10	+ 5	10	−13	18	2.01	4.05
D.M.	10	+ 8	10	− 1	9	1.01	1.01
S.S.	10	−10	10	+ 1	11	1.23	1.51
M.W.	10	+ 9	10	− 3	12	1.34	1.80
W.C.	5	+10	5	− 3	13	2.06	4.23
H.J.	5	+ 6	5	+ 3	3	.48	.23
Total	110	+20	110	−21	41	1.38	$\Sigma\chi^2$=28.82 (12 d.f.) P=.004

1. Some subjects showed a striking preference for the object targets, but they were offset by subjects who got a higher score on the symbol targets. It appeared that the conditions were bringing about unusually large differences between the two kinds of targets in the individual subjects but without any consistent favoring of one kind of target over the other. To measure this tendency, the CR of the difference was obtained for each subject and these CR's were squared

and summed over all 12 subjects for a chi-square test of the distribution of the differences. The sum of chi-square was 28.83 (12 d.f.), with P = .004. This shows that the differences as a set are larger than would ordinarily be found in a chance series, as indicated by the large chi-square. The detailed results shown in Table 1 indicate that one subject (R. B.) showed a "significant" difference between the two kinds of targets and two others (G.H. and W. C.) produced suggestive differences.

2. A further question, however, concerns the way in which the differences came about for the individual subjects as far as the deviation of the two scores from mean chance expectation is concerned. For every subject but one the sign of the deviation was opposite for cards and objects. That is, if the subject scored above "chance" on one he scored below on the other. The probability of finding at least eleven opposite signs of deviation with twelve subjects is .0032.

Discussion

While this experiment does not give significant results to bear out the hypothesis that emotionally toned objects as targets will produce higher scores in ESP tests, it does give some indication that individual subjects show a fairly consistent preference for one kind of target over another. This is in keeping with the view (see 3, p. 126) that a divided project (one with dual targets, dual conditions, or some alternative choice) is more likely to produce significance than a single project.

Although the question with which the experiment was concerned was suggested by the spontaneous case material, it must be conceded that there is a large difference between the kind and degree of emotional situation involved in many of the everyday experiences and that introduced into this laboratory ESP test.

Any explanation for the subject preference in this experiment would be merely conjecture. It might be because the subject simply thought he would score better with objects or with cards, whichever was true in his case. Some of the negative deviations could have been an avoidance effect. There could have been conscious or subconscious unpleasant emotions toward some of the objects or symbols which caused the subject to avoid specific targets. But whatever the reason, the fact that 11 out of the 12 subjects had opposite

deviations between the two sets of targets indicates that psi-hitting and psi-missing were both present in the experiment. This fact may well have been the major complication in this effort to make a test of the effect of emotionally toned targets.

References

1. Carington, W. Experiments on the paranormal cognition of drawings. *Proc. Soc. psych. Res.*, 1944, **47**, 155-228.
2. Fisk, G. W., and West, D. J. ESP tests with erotic symbols. *J. Soc. psych. Res.*, 1955, **38**, 1-7.
3. Rhine, J. B. The problem of psi-missing. *J. Parapsychol.*, 1952, **16**, 90-129.
4. Sinclair, U. *Mental Radio.* Pasadena: Upton Sinclair, 1930.
5. Skibinsky, M. A comparison of names and symbols in a distance ESP test. *J. Parapsychol.*, 1950, **14**, 140-56.
6. Vasse, Christiane, and Vasse, P. ESP tests with French first-grade school children. *J. Parapsychol.*, 1958, **22**, 187-203.

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LETTERS AND COMMENTS

To the Editors:

I am indebted to the editors of the *Journal of Parapsychology* for their compliance with my request that I be provided with a pre-publication copy of Dr. Scriven's paper[1] so that I might prepare a few comments to be published with it. Inasmuch as I became aware of the impending publication of his paper very near to the publication deadline, my remarks must necessarily be brief.

At the outset, I must indicate my substantial agreement with many of Dr. Scriven's views. I see nothing alarming (indeed, I think that parapsychologists should accept it as an ultimate objective) in the absorption of parapsychological phenomena into what is (or will eventually be) considered normal. Differences of opinion about this process have seemed to me to be largely a question of semantics. Knowledge produces a reduction in what is considered to be paranormal while, at the same time, expanding the concept of what is normal. Old concepts lose their worth and old dichotomies disappear. And this is good.

The major fallacy in Dr. Scriven's presentation, as I see it, is based upon a rather common confusion of two questions:

(a) Do parapsychological phenomena (or the potentiality for their production) exist?

(b) How may *opinions* regarding the existence of parapsychological phenomena be effectively modified?

I have the impression that Dr. Scriven is not fully cognizant of the fact that the critic whom he appears to take so seriously has not been at all concerned with the first (and basic) question. Rather, his efforts have been directed toward the modification of opinions about parapsychology.

There appears to me to be quite a simple and valid formula for determining the nature of one's intent insofar as these two questions are concerned. A person who is seriously interested in finding an answer to the first question engages in research himself. The critic under discussion has, by his actions, defined the lengths to which those who are primarily interested in the second question may go.

[1] Dr. Scriven's paper appears on pp. 305-318 of this number of the *Journal*.

It may be difficult for those who know that Dr. Scriven is a man of perspicacity and good-will to understand why he has put so much credence in such propaganda-oriented activities. I think that this is partly based upon the failure of Dr. Scriven to complete his "homework" properly before writing his speech. Information has come to me which indicates that at the time he delivered his address to the Parapsychological Association, Dr. Scriven had not read the rebuttal that Dr. Pratt and I had prepared, although it had been available in print for over two months. It appears likely that, after taking a position before a public gathering, Dr. Scriven did not feel that he could change his basic presentation prior to its publication.

Dr. Scriven has compared our rebuttal unfavorably with our critic's paper on the grounds that our presentation is less "temperate" than his. This brings to mind a sign that I once saw in a restaurant during a relatively affluent period in my life: "Not how cheap, but how good." May I suggest that a proper basis for an evaluation is, "not how temperate, but how valid."

I most heartily concur with Dr. Scriven's suggestion that parapsychologists devote at least part of their research time to the "redoing" of the older, more basic studies. However, a story from the pen of Francis Bacon which I recently saw in the newest edition of Norman Munn's textbook of psychology is apropos:

"In the year of our Lord 1432, there arose a grievous quarrel among the brethren over the number of teeth in the mouth of a horse. For 13 days the disputation raged without ceasing. All the ancient books and chronicles were fetched out, and wonderful and ponderous erudition, such as never before heard in this region, was made manifest. At the beginning of the 14th day, a youthful friar of goodly bearing asked his learned superiors for permission to add a word, and straightway, to the wonderment of the disputants, whose deep wisdom he sore vexed, he beseeched them to unbend in a manner coarse and unheard-of, and to look in the open mouth of a horse and find the answer to their questionings. At this, their dignity being grievously hurt, they waxed exceedingly wroth, and, joining in a mighty uproar, they flew upon him and smote him hip and thigh, and cast him out forthwith. For, said they, surely Satan hath tempted this bold neophyte to declare unholy and unheard-of ways of finding truth contrary to all the teachings of the fathers. After

many days more of grievous strife, the dove of peace sat on the assembly, and they as one man, declaring the problem to be an everlasting mystery because of a grievous dearth of historical and theological evidence, so ordered the same writ down."

For quite a long time now some parapsychologists have been attempting to "count the teeth" in this particular horse's mouth, but the strident cries of the "non-counters" continue to be heard. So I say, let's follow Dr. Scriven's suggestion and re-do the basic studies. But to this, I would add the suggestion that the shrill-voiced "non-counters" descend from their self-created Olympian heights (or wherever else they may reside) and start to do a little "counting" themselves. The method of research is open to all. If, in the unlikely circumstance the participation of these gentry produces a flood of negative results, then so be it. This, after all, is what we want to know.

J. L. Woodruff

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Syosset, N. Y.

To the Editors:

It is very likely that, if correctly treated, the data presented by Dr. C. B. Nash in his article ("The Effect of Subject-Experimenter Attitudes on Clairvoyance Scores," Sept., 1960) would support the conclusion that subject-experimenter attitudes affect scores in clairvoyance tests. The data are not, however, now presented in a form which enables one to discover whether this is so or not. On p. 194, Dr. Nash correctly points out that, to establish such a relation, it is not enough to show that there is a significant difference between total scores obtained for different attitudes. He wrongly supposes, however, that the reality of this relation can be established by treating the work of each subject in each session as the statistical unit of observation and finding out whether there is a significant difference between scores for different attitudes when measured in these units.

This, however, will not do because differences between scoring rates of different subjects (or with different experimenters) may produce a spurious appearance of difference in scoring rates between attitudes if there is a difference in scoring rates between individuals

(either as subjects or as experimenters) and there is any consistency in the subject-experimenter attitude within any particular subject-experimenter pair. The second question is almost certain to be the case and the first is too likely to be ignored.

The simplest way of getting over this difficulty would seem to be to treat each experimenter-subject pair as the statistical unit and then to proceed as Dr. Nash has done with his subject-occasion units. Although better, this would not be free from objection. It would assume that any particular subject (or experimenter) showed no consistency in attitude to different experimenters or subjects. This may well not be the case. Also this method would sacrifice that part of the information given by the effect on the score of any change of attitude within subject-experimenter pairs.

I think the only wholly satisfactory method would be an analysis of variance of the scores obtained by each subject on each occasion, i.e., of the units calculated by Dr. Nash for the purpose of his final tables. This way of treatment would enable one to find out whether a significant amount of the variance between these scores was contributed by other factors (such as differences between subjects and between experimenters). It is to be hoped that this recalculation will be done.

Finally, I should like to suggest, as a matter of editorial policy, that contributors should be asked (where possible) to submit for publication as much of their raw data as may enable any reader so inclined to apply to it a method of analysis other than that used by the author. I know that printing of tabular material is expensive but I find very frustrating the paucity of information in the *Journal* as to what is the raw numerical material from which the author has derived his conclusions.

R. H. Thouless

2 Leys Rd.
Cambridge, England

Editors' Note

When the Nash data are separated into the two rounds of the experiment, the results are as follows:

First Round

	Subject Attitude Positive	Subject Attitude Negative	Total
ESP Positive	22	7	29
ESP Negative	11	18	29
Total	33	25	58

$\chi^2 = 8.51$ (1 d.f.)
$P = .0035$

Second Round

	Subject Attitude Positive	Subject Attitude Negative	Total
ESP Positive	14	13	27
ESP Negative	11	17	28
Total	25	30	55

$\chi^2 = .88$ (1 d.f.)
$P = .3$

This analysis, which overcomes the objection against the pooling of these data in one table as was done in the original report, reveals two points of interest. (1) The relationship between the subject's attitude toward the experimenter and the ESP score level is statistically significant for the first round. (2) This effect is not present to any significant degree in the data of the second round. Thus it seems that the conclusion reached from the research is confirmed and that there is, in addition, a suggestion of a decline effect for the influence of attitude upon ESP from the first half to the second half of the experiment.

The use of analysis of variance with these data is complicated by the fact that the division between positive and negative attitudes is unbalanced. The above analysis seems adequate for the statistical requirements pointed out by Dr. Thouless.

Consideration of *Journal* space and expense imposes limits upon the publication of detailed, original experimental data. The editors regret that it is necessary to operate with this restriction and they hope that any seriously interested students desiring additional information will be able to obtain it by directing their inquiries to the authors of the research reports.

BOOK REVIEW

HIDDEN CHANNELS OF THE MIND by Louisa E. Rhine. Foreword by J. B. Rhine. New York: William Sloane Associates, 1961. 291 pp. $5.00.

At Duke University, in the early thirties, under Professor William McDougall's sponsorship, J. B. Rhine initiated a program of experimental research which led to the establishment of the Duke Parapsychology Laboratory. The stream of publication of the results of the research soon encouraged people from all over the United States, and indeed, from all over the world, to send in their private accounts of queer psychical happenings in the hope of obtaining some sort of explanation and guidance. Thus there have accumulated at Duke several thousand cases of all kinds of spontaneous experiences, and from this large reservoir Louisa E. Rhine has selected and woven into the text of her book over 225 case histories in order to throw light upon the meaning and range of extrasensory perception.

The preparation must have entailed enormous labor spread over several years, for this is no haphazard selection. It is not even a selection of the "best" cases or the more dramatic and startling ones. The author has picked those cases which, after analysis, illustrate the manner and range of nonsensory incursions into the mind. She is trying to find the answers to such questions as: What are the different types of the psi faculty? Who has ESP and how common is it? Do normal people have it or is its manifestation a sign of weakness of character or of some neurosis? What must we think of space and time in psi experiences? Can precognized dangers be avoided? Can mind directly influence matter? Is there communication with the dead?

The book therefore is an escape from artificial laboratory research. It deals with ESP in real life. Therefore, it is not written primarily for the experimentalist, but rather for the untold thousands, of all countries and all cultures, who have psychic experiences, to help them begin to find answers to questions disturbing and perplexing. Some of the answers arrived at are fairly clear and con-

clusive. Others are neither complete nor satisfactory and are left for more research and the better assessment of the facts we already have.

Thus there is no answer to the precognition problem, perhaps the biggest anomaly facing parapsychology. For its occurrence, the evidence seems overwhelming. Eight good cases are narrated in the book. But to the man in the street, as well as to the philosopher, the proposition that an effect will sometimes precede its cause seems just plain nonsense. It is a rather naughty satisfaction to know that today it is not a problem for parapsychology alone, but that even our comrades, the nuclear physicists, are up against a similar perplexity. I understand they have found it necessary to formulate a "reversal of time" in the equations that attempt to cover the movements of subatomic particles and even to state that a nucleus sometimes explodes before it is actually hit by a bombarding particle. It is true in micro-physics we are dealing with intervals of time measured only in millionths of a second, but, as Professor Ducasse has pointed out, whether the effect precedes the cause by split seconds, days, or months, it is surely the same anomaly.

Can the mind influence matter—*outside* the brain as well as within it? There are many curious cases of unexplained effects: falling pictures, broken dishes, disturbed crucifixes, knocks and raps, faucets turned on in the sink, electrical effects, bursting of lamp bulbs, and one most curious case of the feed-back squeal in a hearing-aid to draw attention to the ostensible presence of the departed. There are also four cases of watches or clocks stopping at the precise moment of death of the owner. At the end of the last century there was a popular song, "My Grandfather's Clock," the refrain of which went:

> Ninety years without slumbering, Tick-tock, tick-tock,
> His life's seconds numbering, Tick-tock, tick-tock,
> But it stopped short, never to go again,
> When the old man died.

It would be interesting to know if the writer of the "lyric" had had personal experience of something of the sort and, in the following century, was it this song that triggered off the psychic experiences which were apparently quite numerous? The movement of pendu-

lums has been the subject of laboratory PK experiments but, I believe, without much success. It might be well worth while to investigate further this "natural" phenomenon. If the mind can influence the fall of dice, why not the movement of the more sensitive pendulum?

Before the reader has turned many pages it is inevitable he should ask himself the very proper questions—How much reliance is to be placed on all this anecdotal material? To what extent are the facts distorted by human errors, preconceived ideas, malobservation, forgetfulness of important details, dramatic exaggerations, deliberate fraud, and so forth and so forth? Such errors *must* exist but to what extent? What "margin of error" must be allowed for in any case experience to discover its final real value as evidence of the paranormal? The author does not shrink from facing the problem. In her view validity is created by the continually growing numbers of similar items. Her argument is that while a single experience of any given kind is unconvincing, a hundred or more of the same type are not to be brushed off so easily. There is an accumulative effect. Similarities show up from widely different sources so that one can begin to see in the background a rationale that could hardly result from a series of mistakes. For if they are all mistakes, why the pattern?

To your reviewer that is the crux of the problem. The pattern itself is the phenomenon to be explored. The universality of stories of haunts, apparitions, forebodings, extrasensory contacts—in short, psi phenomena throughout the ages, with all sorts of peoples and cultures—is a phenomenon of unmistakable significance. But significant of what? Is it simply a pattern of man's infinite capacity for self-illusionment, or does it exhibit the design of intrusion from a greater Unknown?

It is easier to decide between these alternatives today than it was thirty years ago. The trend of research is turning full circle. Originally the pioneers were concerned mainly in recording, analyzing, and theorizing on the spontaneous manifestations of psychical phenomena. Experimentation had barely begun although it should not be forgotten (as I am afraid it is) that some very good experiments with positive results date from the earlier days. But things developed, and soon the new techniques and successes of Dr Rhine

and his colleagues at the new Parapsychology Laboratory inspired co-workers in every country where research was being pursued. There even grew up a kind of rivalry, if not a latent antagonism, between the old "anecdotalists" and the new "experimenters." The non-mathematically-inclined anecdotalists viewed the critical ratios and standard deviations of the newer group with suspicion and dislike. The experimenters tended to regard the little tales of the orthodox with an equal disdain. It was some time before the complementary nature of the relationship of the two groups was fully realized. But now the wheel is turning and, as this book reveals, a new and greatly improved situation has emerged. Each group has discovered its dependence on the other. Before the days of organized experiment the burden of the proof for the reality of psi rested on the little tales. An all-convincing proof was never forthcoming. But today, we have in addition the accumulating evidence from the work of the laboratory group. The anecdotalists are relieved of much of their original duty. They need no longer try to perform the impossible task of deducing from spontaneous cases mathematically valid proofs of psi's actuality. They can leave that to their brothers, the chi-square lovers, and devote their energies to the collecting, sifting, dissecting, collating of more and more case histories without being sidetracked by questions of their fundamental validity. This, of course, does not mean there may be any relaxing of the "standards of evidence." On the contrary, the field-naturalist, with more and more experience to warn and guide him, more than ever will be alert to spot the signs of distortion and human error.

But if we are looking for final proof, have the experimenters succeeded in providing it? For some of us the answer is Yes—especially for the fortunate ones who have had successes in their own experimental work quite insignificant as that may appear to outsiders. But for others, in fact for the majority in the scientific world, the matter is still not proven. Why is this? I think Professor Sir Cyril Burt recently has made a wise statement:

> By placing the emphasis on the paranormal character of the phenomena instead of on their psychological character, students of psychical research have to some extent impeded the scientific study of the problems concerned and delayed the acceptance of results already achieved.

Have we not to consider that we have been using a wrong emphasis, even in our experimental work? Quite apart from our treatment of spontaneous cases? Have we not over-emphasized the importance of *proof*? As Dr. Thouless recently pointed out:

> No single experiment can be conclusive since the experimenter may have cheated . . . my conclusion is that since no single experiment can be conclusive we should give up the attempt . . . I would go further and say that the purpose of an experiment should be to solve a problem and not convince anybody. That this is the aim of an experiment would be taken for granted in any other experimental science, as for example, in experimental psychology which is the field with which I am most familiar. I think that where experimenting often goes wrong in parapsychology is because those not familiar with other fields make the mistake of thinking the object of an experiment is to convince rather than to solve a problem.

I suggest that we cease pursuing the chimera of absolute proof. Let the George Prices and Hansels stew for a while—not in their own juice for that would be too inhuman, but in a distillate from all the best psychical research of the century. Prolonged stewing tends to soften the toughest tissue. And let the experimenter seek to improve his techniques to lessen any equivocalness in his results, and simply aim at solving the succession of problems that arise in the course of his work. And let the field-naturalist go on discovering and cataloging the specimens he culls from the complexities of human psychical behavior. In the end parapsychology, if it is found worthy, will win its recognition.

We are living in an expanding universe. In the last fifty years our knowledge of the material world has increased with frightening speed. It is a far cry from Galileo and his spy-glass, with which it was just possible to make out the moons of Jupiter, to the gigantic eye on Mount Palomar with which, one thought, astronomers had pierced the greatest possible depths of space. And then, before we could recover our breath, a new technique burst upon us, radio astronomy, by which soundings are being taken of even vaster depths. And now, in another direction, are there not the beginnings of a parallel expansion to be faced? Are we not knocking at the portals of mind and consciousness and receiving strange answers? Two hundred years ago William Blake wrote: "Man has closed himself up, till he sees all things thro' narrow chinks of his cavern." But

today, through the "hidden channels of the mind," man is beginning to unfold himself and the narrow chinks of his cavern are being replaced by "magic casements" opening out to new horizons. The human spirit is not to be confined within mere sensory limits. That much at least is now clear and sure. We shall all of us have to be alert, preparing to change old narrow outlooks. So much the better. To quote Blake again: "The man who never alters his opinion is like standing water and breeds reptiles of the brain." This book should be an effective contraceptive for ***reptilis mentis***.

G. W. Fisk

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Surrey, England

THE PARAPSYCHOLOGICAL ASSOCIATION

THE FOURTH ANNUAL CONVENTION

The fourth annual convention of the Parapsychological Association was held at the Henry Hudson Hotel, New York City, September 7-9, 1961.

PROGRAM

Thursday, September 7

I.

9:30 Registration

10:30 Convention Call to Order. Announcements.....

Karlis Osis, President

11:00 Research Briefs. Chairman: J. G. Pratt (Duke University)

E. M. Holleran (St. John's University): Studies in progress on the effect of PK on objects (dice, spheres, pendulum, etc.), on radiation, chemical reaction rates, and the growth of plants and bacteria.

Bernard Grad (McGill University): A description of experiments on the germination and growth of barley seeds which were "treated" by a healer.

Brenio Onetto (University Psychiatric Clinic, Santiago): Exploratory tests at Duke University on the effect of a stimulant drug (methylphenidate) on ESP ability.

Lawrence Casler (Columbia University): Report on experiments to "liberate" ESP by the use of suggestion under hypnosis.

W. R. Clement (Parsons College): Studies in progress on the relation between anxiety and the communications aspects of ESP.

Robert L. Taetzsch (Middlesex, N. J.): Results obtained from exploratory experiments with the psi communications system described at last year's convention.

John Freeman (Duke University): Description of tests of precognition in which the subjects were encouraged to believe they could score more easily on the precognition part of the experiment than on the clairvoyance part.

W. E. Cox (Southern Pines, N. C.): Request for accounts of poltergeist cases to be used in a proposed survey and analysis.

Stephen Abrams (Oxford University): Description of proposed psi experiments stressing multivariate analysis to be carried out by a group of parapsychologists at Oxford.

Jule Eisenbud (University of Colorado): Report of research based on the hypothesis that psi and subliminal perception reinforce each other, the results indicating that when one was in operation the other switched off.

David Techter (Chicago Museum of Natural History): Report on the activities of the Illinois Society for Psychic Research.

Douglas Dean (Parapsychology Foundation): Exploratory tests of the induction of hypnosis at a distance, along lines similar to work carried out in Russia in the thirties.

II.

Stimulus Variables and ESP

Chairman: Margaret Anderson (University of Pittsburgh)

2:00 "ESP and Size of Target Symbols" by Rémy Chauvin (Laboratoire d'Ethologie Experimentale, Paris)[1]

[This paper is published in full on pages 185-89 of the September number of the *Journal.*]

2:25 "An ESP Test Involving Emotionally Toned Objects" by John Freeman (Duke University)

[This paper is published in full on pages 260-65 of this number of the *Journal.*]

2:50 "A Comparison of Responses to Extrasensory and Subliminal Targets" by C. S. Nash (St. Joseph's College)

Two high-aim runs and two low-aim runs with randomly arranged subliminal and blank targets were given to each of 45 subjects. The name of one of the five ESP symbols was written on

[1] In the author's absence, a summary of this paper was presented by Dr. J. G. Pratt. Copies of the complete paper were made available to the audience.

the frame of each slide, whether blank or subliminal, and was not visible to the subject. Any extrachance scoring on the blank slides would, presumably, be due to ESP. The subliminal targets were exposed to view for .01 seconds. The subjects were told that all of the targets were subliminal and were instructed to check those of their calls which they believed to be hits in the high-aim runs. Scoring on the ESP targets was at the chance level for both high and low aim; on the subliminal targets, it was significantly positive for high aim and significantly negative for low aim. Checking success in the high aim was greater in the first than in the second of the two runs, a difference with a P of $< .01$. Response to subliminal targets was seen to resemble response to ESP targets with respect to suggestive inter-run and intra-run declines and with respect to the ratio of the deviations between high and low aim.

3:15 "ESP Response to Preferred Targets as Compared to ESP Symbols" by K. Ramakrishna Rao (Andhra University, India)[2]

The subjects in this experiment were asked to choose five symbols which had a favorable emotional significance to them, and to inscribe these symbols on cards provided for this purpose. All of the six subjects did better with the cards of their choice than with standard ESP cards. In a total of 100 runs—half of which were carried out with the "choice" cards, and half with ESP cards—the experiment yielded a CR of the difference of 3.1 in favor of the hypothesis that the subjects would respond better to cards of their choice than to the symbols on the ESP cards.

3:30 Coffee. Intermission.

4:00 Presidential Address by Karlis Osis (Parapsychology Foundation)

[The full text of this address appears on pp. 290-304 of this number of the *Journal.*]

5:15 Council Meeting

[2] In the author's absence, a brief description of this paper was presented by Dr. John Freeman. Copies of the complete paper were made available to the audience. This experiment was performed at Duke University in 1960 while the author was a Visiting Research Fellow in the Parapsychology Laboratory.

Friday, September 8

I.

Subject Variables and ESP, Section I.
Chairman: Gardner Murphy (Menninger Clinic)

9:00 "The Plethysmograph as an Indicator of ESP" by Douglas Dean (Parapsychology Foundation)

In this experiment a relaxed percipient was in one room with his right forefinger attached to the finger cup of a plethysmograph. The plethysmograph and an "agent" or sender were in a different room. At randomized time intervals unknown to the percipient, the agent looked at randomized cards made up of five types of stimuli: names of persons known only to the percipient, names of persons known only to the agent, names of persons known to both agent and percipient, names unknown to either, and blanks containing no names. Dips in the curve on the plethysmograph record corresponding to stimulus periods were measured by a person who had had no access to the data. A statistical analysis was carried out on the vertical drop of the curve during the dip. In the case of the subject Mr. B. (the only one for whom there are sufficient data) the mean deviation for names known to percipient-agent over no-name stimuli gave a $t = 4.0$ ($P = .001$). The deviation for names known only to the percipient over no-name stimuli gave $t = 2.4$ ($P = .01$). Names known only to the agent and names unknown to either gave deviations that were not significant.

9:25 "Testing for ESP by Comparison of Machine Recorded Streams of Thought" by B. C. Kirby (San Diego State College)

A series of efforts were made to test for telepathy by comparing machine-recorded streams of thought, spoken simultaneously by an agent and one or more percipients, in order to discover what, if any, similarities there might be in the agent's and percipient's records. The method of evaluating the material was to have independent judges try to match the records of agents and percipients. In this manner, relatively complete slices of consciousness may be examined carefully by judges sensitive not only to obvious parallels in events

or objects, but also to parallels obscured by distortion or the sometimes symbolic language of the subconscious. In several series, involving 44 percipient-subjects and three agent-subjects, 10 judges were not able to match the records with more than chance success. Several possible reasons for the negative results have been considered. One of these is related to the efforts to eliminate time clues. In order to exclude rational inference from time cues on the part of the judges, somewhat unsatisfactory means were adopted in the testing procedure.

9:50 "The Psychological Process in ESP Experiences" by L. E. Rhine (Duke University)

One place to look for hints as to the nature of the psychological side of the psi process is in spontaneous experiences that seem to involve ESP. Among these, instances in which an incomplete item of information is secured are particularly suggestive. The kinds of incompleteness are found to differ characteristically in each of the four forms of spontaneous experience (realistic, unrealistic, hallucinatory, and intuitive). Two hundred and fifty-eight instances of imperfect realistic dreams were studied to see what appeared to be the reason why the complete and correct information had not been secured as in the majority of realistic dreams. It appeared that the personality of the percipient had modified it in accordance with unconscious motivations connected with his emotional relationship with the agent. The study suggested that the psychological process by which the imagery of realistic dreams comes into being is not that of an automatic reproduction of reality but that, instead, the personality can leave its imprint on it.

10:15 "Student Preferences Shown in a Two-Classroom GESP Experiment with Two Student-Agents Acting Simultaneously" by Rhea White and Jean Angstadt (American Society for Psychical Research)

The 55 students in each of two high-school biology classes (held during Periods 1 and 5) selected by secret ballot the student in each group whom the majority of each class felt closest to and liked best. These two representatives simultaneously and during both

periods looked at different sets of 20 playing cards while the other students in the two classes, respectively, tried to guess the cards at which their own representative was looking. The object of the experiment was to see if the students in each period would be able to score higher on the targets of their own representative than on the targets of the other representative. The subjects were aware that there were two representatives and that they would be eligible for a prize only if they scored higher on their own representative's targets.

Both classes together scored significantly higher on their own representative's targets ($P = .0008$); but Period 1 subjects scored higher than Period 5 subjects. The scores on the targets of the Period 1 agent are significant ($P = .002$); those on the targets of the Period 5 agent are not. When boys are compared with girls, only the girls' results are significant ($P = .002$). However, in both classes there was a tendency for girls to score higher if the preferred agent was a boy, and for boys to score higher if the preferred agent was a girl.

Subject Variables and ESP, Section II.
Chairman: Julé Eisenbud (University of Colorado)

11:00 "Retest of High-scoring Subjects at Varying Distances" by **Carroll B. Nash (St. Joseph's College)**

Thirty-nine of the 90 contestants with the most nearly correct entries in the Chesebrough-Pond ESP television contest of 1958 were given from one to five ESP runs made up of the digits 0-9 arranged in random order. The recorded targets were kept at the Parapsychology Laboratory at St. Joseph's College, Philadelphia, and the calls were submitted on postal cards. The total deviation was negative, and the results suggest that the ESP scoring level is not affected by distance, at least as far as 2500 miles.

11:25 "A Comparison of Scores on an ESP Test and on Psychological Tests" by G. R. Schmeidler (City College of New York)

An increasing body of research indicates a relation between personality trends and ways of perceiving. One such relation which

has been frequently reported with adult subjects is that individuals who are "self-inward" and rely on repression as a means of defense tend to be "levelers" rather than "sharpeners" in their perceptual judgments; that is, they tend not to observe small stimulus differences.

To find whether similar relationships occur in children, 49 children with a mean age of 11 years were given a personality test, two tests of perception, and an ESP test consisting of 36 "clock" cards. IQ's were available for 23 of the children.

When scores on the tests were correlated, nine of the ten coefficients were in the predicted direction. ESP scores showed a significant correlation with one of the perception tests ($P = .01$) and a suggestive correlation with mental age ($P = .05$).

Thirty of the children lived in an orphanage; 19 lived in their own homes. The orphanage children tended to be more self-inward (P of the difference $= .01$) and to have lower ESP scores (P of the difference $= .025$) than the children living at home.

11:50 "Sensory Deprivation and Séance Phenomena" by J. H. Rush (Boulder, Colorado)[8]

Experiments at McGill University and elsewhere have demonstrated that persons whose sensory inputs are limited to monotonous, informationless stimuli undergo significant mental changes. Perceptual judgment and time sense are impaired, concentrated rational thinking becomes difficult or impossible, suggestibility is enhanced, and illusions and hallucinations develop. Broad individual variations in susceptibility to such effects are observed.

Mediumistic séances typically have involved conditions approaching those of the experiments in sensory deprivation. Sitters wait in dim light or darkness, voices are subdued, distracting (i.e., stimulating) conversation is discouraged. Sometimes the sitting proceeds in this way for several hours. Critical investigators have long recognized the lapses of attention, increase in suggestibility, and uncertainty in visual perceptions under such conditions, but they have not appreciated the extent and subtlety of such effects. The results of the experiments in sensory deprivation require a critical reassessment of many séance observations.

[8] In Dr. Rush's absence, his paper was read by H. A. Cahn, Syracuse University.

On the positive side, there are reasons for believing that these effects, so detrimental to critical, rational functions, may facilitate psi experiences. The sensory deprivation experiments suggest a possible experimental approach to the investigation of séance phenomena, somewhat as the card-guessing techniques put "mind reading" on an experimental basis.

II.

Precognition

Chairman: C. J. Ducasse (Brown University)

2:00 "An Asymmetrical Precognition Experiment Using ESP Cards" by W. E. Cox (Southern Pines, N. C.)

The subjects in a precognition experiment were asked to fill in their calls in alternate (call) columns of the record sheet without being told that when the target symbols were later put into the adjacent (card) columns an unusual method of checking the hits in the odd-numbered columns was to be used. The pairs of call-and-target columns are numbered from left to right on the record sheet. The targets for the even-numbered columns were filled in first, on the basis of random number tables. Then the preceding odd-numbered columns of targets were filled in by a routine system which depended on both the symbol and location involved in the hits in the immediately following (even-numbered) column. The results, though not significant, tended to favor the odd-numbered columns. Thus the author's purpose of testing whether precognitive success is unaffected by calculated intervention as appears to be true of some spontaneous cases was not fully achieved, but the results encourage further work along the same lines.

2:25 "A Comparison of Verbal and Written Responses in a Precognition Test" by Michael Sanders (Duke University)[4]

A pilot study carried out with 20 female undergraduates gave a significant difference in the rate of scoring between verbal and written responses in a precognition test. A confirmatory experiment was conducted with a two-experimenter set-up. The same 20 stu-

[4] In the author's absence, a summary of this paper was presented by Mr. Alan Mayne. Copies of the complete paper were made available to the audience.

dents were used in the confirmatory series. Each did two sessions of four runs each, in two of which she wrote her responses and in two of which she called them. Before the first run, it was noted whether the subject preferred to speak or to write her responses. The differences in ESP results between the two methods for the individual subjects showed a significant departure from the expected random distribution ($P = .005$). Also, the series as a whole showed significantly higher scoring on the preferred method (P of the difference = .0055).

2:40 "Precognition in a Dream Series" (Film) by Hans Bender (Institüt fur Grenzgebiete der Psychologie und Psychohygiene, Freiburg, Germany)

A documentary film entitled "The Case of Gotenhafen" presented the evidence for precognition in a group of 12 dreams experienced by a German movie actress. Beginning in 1954, about one thousand dreams from this actress were placed on record in the Freiburg Institute. The twelve which were the subject of special study were selected because they appeared to contain many references to events which took place several years later, during the filming of two movies in 1959. The documentary material consists of film clips taken from the two movies, interviews with witnesses, reconstruction of events which occurred during the filming, and excerpts from the actress's diary. The evidence is examined by the investigators from both factual and psychological points of view, especially in regard to the linking of the contents of the dream and the subject's situation at the time the dream occurred and at the time of the apparent future fulfillment of the dream.

3:35 Informal Panel Discussion on ESP and the School. Panel Members: Margaret Anderson (Chairman), Mark Rilling, Rhea White

III.

7:00 Convention Banquet. Address: "New Frontiers of the Brain" by Michael Scriven (Indiana University)

[The full text of this address is presented on pp. 305-18 of this number of the *Journal.*]

Saturday, September 9

I.

Novel Techniques in ESP and PK

Chairman: R. A. McConnell (University of Pittsburgh)

9:00 "Statistical Methods for Evaluating ESP Tests on Qualitative Material" by A. J. Mayne (Leeds University, England)

An alternative approach is proposed to the existing methods for assessing qualitative ESP data. The range of qualitative material that is used in the ESP test is divided into one or more categories (for instance, a test on famous people might involve the categories of their sex and geographical region). The choices possible within each of these categories are then arranged in random order and suitable qualitative targets are then assigned to them. Thus a multiple scheme of categories can be used in which each specific "target" is assigned to fit several categories. The results are scored for statistical purposes in terms of the random categories rather than exact hits. Several examples are given of procedures for choosing qualitative targets at random and for scoring hits on them. In general, suitable scores are assigned to varying degrees of "near miss" or "partial hit" as well as to "direct hit." The statistical significance of such scores can be evaluated with reasonable ease and accuracy by recently devised methods. With skillful planning this procedure should be applicable to a wide range of target materials.

9:25 "A Case Study of Animal Behavior Known as 'Psi-Trailing' " by Sara R. Feather and J. B. Rhine (Duke University)

From a collection of reports of unusual animal behavior which suggest a psi factor, 54 "psi-trailing" cases were selected for study. These are instances in which an animal, separated from a person (or mate) to whom it has become attached, reportedly follows the departed companion into wholly unfamiliar territory under conditions that would allow no conceivable sensory trail to be of possible use. If these occur as reported, this type of phenomenon represents the

most unequivocal case for animal psi in all spontaneous behavior reported. A working chart of criteria governing the value of such cases was developed with emphasis on the importance of the identification of the animal, and three of the cases that best meet these standards were presented.

Difficulties encountered in the experimental testing of the question of psi in animals have led to the need for a new intermediate methodology to bridge the gap between spontaneous cases and laboratory tests. Some specific suggestions for semi-experimental projects have come from studying the psi-trailing cases, and continued value from such case studies is anticipated.

9:50 "A PK Experiment with Doubles as Targets and Non-Targets" by Haakon Forwald (Ludvika, Sweden)[5]

A PK experiment using dice was carried out in the following manner: In Part I (the "target" section), the subject threw two dice at a time onto an enclosed throwing surface, attempting by PK to make any pair of doubles turn up. In Part II (the "non-target" section), he tried to avoid throwing doubles. The first of the subseries in Part I was followed by the first subseries in Part II, and so on through the experiment, in order to try to avoid a chronological decline effect. In Part I, it took the subject only 5206 throws to obtain the goal of 960 doubles ("chance" would be 5760 throws), and in Part II, where he was trying to avoid doubles, it took 6174 throws. The difference between the number of throws in Part I and Part II is highly significant (P = approximately .00006). There were distinct position effects in the results that were related to the experimental design.

10:30 Panel Discussion on Post-Mortem Survival.
Chairman: W. G. Roll (Psychical Research Foundation)

"Methods of Investigating Some Alleged Cases of Reincarnation in India" by Dr. Ian Stevenson (University of Virginia School of Medicine)

[5] In the author's absence, a summary of this paper was presented by Dr. R. A. McConnell. Copies of the complete paper were made available to the audience.

More than 40 cases suggestive of reincarnation in India and Ceylon have come to the attention of the investigators. In the summer of 1961, 21 of these cases were studied—some only briefly and some in considerable detail. The cases had all occurred in children. Primary witnesses in the family and community where the child now lives and in the community where he claimed to have lived previously were questioned. The testimony of these persons was concerned with the facts related to the statements of the child and to the recognition by the child of objects and persons of that community if he had visited it. The different accounts of the child's statements and of the various procedures of verification were checked against each other. Accounts of different persons and of the same person at different times were found on the whole to be quite stable, although some discrepancies occurred. The investigators directed inquiries with a view to elucidating the possibilities (and motives) of fraud. Their inquiries also focused on the possibilities of communication through normal means of the information apparently remembered by the child. Attention was also paid to details of the apparent memories that might assist in estimating the likelihood that the child's information was obtained by ESP.

"Methodological Problems of Experiments with Mediums" by Karlis Osis (Parapsychology Foundation)

The emphasis in research with mediums during the last decades has been placed on: (a) designing a "crucial" experiment for post-mortem survival; and (b) estimating statistical significance in deviations from chance distribution of verified mediumistic statements, culminating in the Pratt-Birge method. The concept of the one "crucial" experiment has brought this area of research to an impasse. It is unsound as research strategy, foreign to current experimental design, and has led to efforts resulting only in failure. The Pratt-Birge method is a useful statistical tool, but it gives uninteresting information. It tells us whether something of an extrachance nature is present or not in the experiment; it does not enable us to analyze what this "something" really is. The problem of real importance is how to measure the various parameters and their interactions in mediumistic data, especially those relevant to the ESP hypothesis

and to the survival hypothesis. Modern multivariate experimental designs and appropriate statistical assessment are necessities in research on mediumship. In the mediumistic sessions of pioneer times an abundant flow of psi imagery was often obtained. In recent years a one-sided pre-occupation with complex technicalities has drained this natural well-spring. We should apply our best psychological understanding to the test situation and pay much more attention to the subject as an individual in order to regain such apparently rich results.

"An Investigation Relevant to the Problem of Post-Mortem Survival" by W. G. Roll (Psychical Research Foundation)

A series of experiments were discussed in which a hypnotized subject, a housewife from Richmond, Virginia, was requested to communicate with incorporeal personal agents (IPA's). The subject was asked to provide detailed information allowing verification through death certificates, newspaper obituaries, and similar sources and through personal circumstances verifiable only through relatives and associates of the deceased. Of 54 alleged IPA's who have been described by the subject during the past year, 23 have been traced in existing records. One of these (the Hudson case), in which the identification was unmistakable, was selected for special study. Two counterhypotheses to a parapsychological explanation of this case were considered: unconscious memory and conscious deceit. To test the former, a hypnotic memory interview with the subject was conducted by a person not otherwise connected with this research. Suggestions for truthfulness and improvement in the subject's memory were given. With regard to the fraud hypothesis, two types of tests were conducted, one for the genuineness of the hypnotic state and the other a polygraph test for "lie detection." Both tests indicated the absence of any conscious efforts to practice deceit. These interviews and tests support a parapsychological interpretation of the Hudson case.

12:00 Announcements and Adjournment . . . Karlis Osis, President

ESP IN THE LABORATORY: ITS INTEGRATION AND ACTIVATION[1]

By KARLIS OSIS

ESP IS AN integral part of personality. Its activation depends on interpersonal fields as well as on situational and cultural variables. In life situations it emerges when proper constellations of these complex wholes spontaneously occur. Do we need to recognize such constellations as a necessity in our experiments? In the laboratory, is ESP embedded in natural soil, i.e., integrated with the important variables necessary for its occurrence? Let us discuss this specific problem within the broader context of the present situation of parapsychological research.

Like any science, parapsychology has had periods when there was a dominant methodology accepted by the majority, and also periods of radical reorientation and even confusion. Viewing the present research scene, one must admit that parapsychology is at a crossroads. Part of the reason for this, of course, is the shift of generations in the research field. We have many new workers, trained in modern experimental techniques. Research methodology in psychology has undergone unbelievably rapid developments since the thirties, and this is necessarily reflected in ESP experiments.

However, the historical situation in psychical research has sharpened the radicalism of the new generation. The decade of the thirties was quite revolutionary as compared with the twenties, and it gave us the simple, clear methodology of card-guessing techniques. Experiments looked so simple! Undergraduates could run them—and did. A fair appraisal shows that many important contributions to knowledge were made and splendid research projects emerged from this type of experimentation. If there had been no radical departure from the methodology of the twenties, vast areas of parapsychology would still remain unexplored. So far, so good. But major frustrations were also created by this simple methodology. I feel we have now reached the stage of maturation where we can

[1] Presidential Address presented on Sept. 8, 1961, at the fourth annual convention of the Parapsychological Association in New York City.

discuss them candidly and constructively, without arousing the usual defense mechanisms familiar to psychology.

One problem was that experimental results soon dwindled to extremely small margins. Average deviations dropped from a 1-to-5 range in the early thirties to .1-to-.5 in later publications, including my own. This was a fact we had to face and accept. But it was a bitter one, because many more counterhypotheses are applicable to small effects than to those of the magnitude achieved by Pearce or Riess's subject. This situation could be met adequately only with extreme rigor in experimental conditions.

Parapsychologists became aware that minute slips, such as slight recording errors—perhaps one in five hundred trials—which could be disregarded in experiments on more sizeable variables, might tip the scale when only tiny bits of information were being transmitted. Other dangerous pitfalls which increased as average scores declined included possible nonrandomness of stimuli, inclusion or exclusion of results from initial testing, significance tests chosen after inspection of data, improper use of parametric statistics, and so on. The problem of research on small effects is met by adequate experimental designs in general psychology as well as in psychical research. In my opinion, good parapsychological experiments have, in fact, effectively eliminated reasonable counterhypotheses.

Research on small effects can, definitely, be reliable, but the large area of possible defects creates a wide, foggy zone of uncertainty between what is acceptable and what should be rejected. Unfortunately, the slippery danger areas of research on small effects are not easily defined or assessed. The criteria of what is acceptable and what is not vary widely among well-informed and sincere persons. For example, two past presidents of the Parapsychological Association published a book in which the main effects described had an average deviation ranging from .1 to .2 It is my personal opinion that their book is an admirable, peak achievement in parapsychological research. Another brilliant experimenter (one who was in charge of S.P.R. research) tried many ingenious tests, and denounced the results in all of them as unsuccessful. When I pointed out the high average scores on some of his tests, his answer was startling: he considers an average score of 6 as necessary to overcome reasonable counterhypotheses. Apparently his standard was ten times higher

than that of the book I have mentioned. This illustrates that there is a fearfully wide zone of disagreement. I personally feel that it could be considerably reduced by the sheer use of common sense.

In other sciences an important safety device for errors in small-effect research is repetition: repeated series with varied methods and experimenters. Unfortunately this particular safety device works poorly in our field. We must remember that for most parapsychological workers, the majority of their experiments do not work out; that is, one finds no trace of ESP in them. As an extreme illustration, I have seen a survey in one area of ESP research where only four per cent of the experiments were successful and accepted for publication. Fortunately, we are doing much better in most areas of our inquiry, but this is not exactly a situation which evokes trust in the results as to small effects. I am not sure about ghosts in general, but the ghosts of unsuccessful experiments are very real, and they haunt the vast graveyards of our field.

We are also haunted by what might be called the ghost of *non-repeatability,* for ESP experiments are not easily repeated even by their authors, and rarely repeated by other, independent researchers. I located the first appearance of the term "repeatability" in S.P.R. publications, as listed in the Combined Index, in 1936. In 1942 an article by Pope and Pratt brought it up in the *Journal of Parapsychology.* Later on Gardner Murphy demanded it with desperate seriousness, and if it could not be achieved, suggested shifting emphasis from experiments to case work.

Is the repeated experiment really as rare as it is assumed to be? I surveyed the last five years of the *Journal of Parapsychology.* It is somewhat subjective to classify an experiment as repeated, since in the repetition one aspect of the experiment may be repeated while another is not. In my count of about 48 experimental papers, only five were repetitions by other experimenters without involvement of the original authors. Four of these gave confirmatory results. This brings up a thorny problem which provides a tempting basis for verbal fencing in editorials and articles on methodology.

Needless to say, the weaknesses in our field have been exposed to exaggerated and emotional attacks by noisy spirits other than poltergeists. Ridiculous accusations have been leveled at us, including comparisons with flying saucers, etc. It is disheartening

to read "critical" papers which are only ten percent matter-of-fact talk mixed with ninety percent braggadocio—sweeping statements whose "brillance" mirrors the author's personality. One wonders if a quite simple psychological mechanism called "displacement" in psychoanalysis may not be at work. The *Dictionary of Psychology* defines it thus: "the transference of affect from one idea to another, usually to a logically inappropriate idea, but quite logical and appropriate for unconscious thinking . . . first observed in dreams, but later discovered to operate generally, particularly in obsessions and perversions." Naturally it will not contribute to the progress of science if stored-up frustrations and aggressions caused in private life, e.g., by mother-in-law, boss, or girl friend, are translated into parapsychological terms.

After these long, heated attacks have subsided, we may conclude that they have not destroyed parapsychology, of course; but neither have they left it unchanged. In the midst of many unreasonable demands, some constructive criticisms have been made. I believe Dr. R. H. Thouless expressed the general feeling among parapsychologists in his address to the Association when he said: "But better than arguing is to develop a line of research to which such objections do not apply (7, p. 9). The general feeling among research workers is not that we are forced to retreat but that for better progress we must make changes. As Gardner Murphy predicted at our first convention, many methodological departures from the simple card-guessing techniques have been originated. For instance, let us remember the ingenious procedures and model building by Scott, Cadoret, and Turner, Thouless' repeated-guessing technique, and Taetszch's sequential analysis by means of the electronic brain. In addition there have been the plethysmograph work of Figar and Dean, Ullman's dream experiments, the Saskatchewan experiments with twins, the Parapsychology Foundation's ventures in modern pharmacology, and Pratt's ESP experiments with birds. There is a fresh breeze of ingenuity sweeping the barren fields in many directions creating many promising currents in the air.

I shall be concerned with an aspect of ESP that is analogous to the cultivation of flowers or shrubs in a garden. Anyone brought up on a farm or in suburbia is very much aware of the basic need of all living things to be properly planted in a satisfactory environment.

We would expect the plant to grow well if the soil is of good quality and adequately prepared, if there is not too much or too little light and moisture, and not too ferocious competition with weeds. Now if the gardener is concerned with only one of these many, many factors—if for example he concentrates on soil quality but ignores the need for water and light—the plants' growth will be very uncertain. Some will survive; most will perish.

I have at home a six-year-old gardener who at one time was extremely interested in one single root of a shrub. In fact, it concerned her so much that she forgot about all the other parts of the shrub. With the utmost care she planted only this one root, leaving all the others exposed to the sun. To my amazement the shrub did manage to grow, but would you expect other shrubs to grow under such literally one-sided conditions? In other words, obsession with one factor may give acceptable results, but we cannot expect them to be repeatable if the total situation is utterly neglected. I am afraid we experimental parapsychologists have sometimes been as one-sided in our way as my little gardener at home.

It is self-evident that ESP is part of the integrated, complex whole of an individual, his interpersonal relationships and the culture he comes from; and vast troubles arise from neglecting to consider this fact in experimental designs.

ESP captures our imagination; a bright aura of fascination surrounds it. We become involved, enthusiastic, or even passionate about it. By quite simple psychological mechanisms, our fascination with the whole is transferred to the tiny problem we are experimenting with. When this mildly obsessional state occurs, the problems become distorted and out of perspective.

Of course psychical researchers recognized very early that the nature of psi functions was a fully integral part of the human personality. Frederic Myers' book is a great classical attempt at a theory of psychical phenomena as woven into the general concept of personality of his time (4). Gardner Murphy has also stressed the same point, but within the framework of modern psychology (2). As J. B. Rhine says picturesquely: "The science of psi is not an island. New though it may be to the explorer, it has connections with the mainland; it is really one section of a vastly larger order that has many and varied parts" (5, p. 147). This is indeed true,

but when we leave the realm of thinking and come to practical ***experimentation,*** why does our broadmindedness shrink?

Let us invent a researcher. He will be neither at the top nor at the bottom in quality. His habits will mirror the average. Let us call him Mr. Average. Suppose Mr. Average has a well-chosen stimulus variable to work with, which he formulates in a question: Will the ESP cards do as well as color cards or cards with emotionally-loaded pictures? This problem is real and worth while. It has from time to time, when their experiments are repeatedly unsuccessful, haunted the minds of many researchers. An impatient attitude might be thus verbalized: "To Hell with the dull ESP cards!"

Mr. Average's experiment, of course, will be very proper. It will be safeguarded against sensory clues. The design will include the proper checks against bias, using all three kinds of stimuli on all subjects at one occasion, and rotating the order of their presentation throughout the experiment. A watchdog of appropriate breed will also be included so that the experimenter can be spared the temptation to cheat.

By the time all the details are organized, the poor experimenter's mind is so filled with technicalities and with the fascinating aura of his problem that many other important variables determining ESP are not considered at all.

Who will be his subjects? There is a rich body of experimental knowledge as to who is good for what in ESP experiments. Mangan's monograph alone has 57 references (1). Schmeidler and McConnell's book has 71 references (6), and there have been more publications recently on this topic. If we are familiar with this extensive literature, we must surely say that appropriate selection of subjects is important. Indeed this factor alone might determine the success or failure of the experiment.

Our Mr. Average has read about this. He knows quite well that several studies using different tests have produced solid findings: in general, maladjusted, unstable or neurotic persons will adapt poorly to the test situation, and their scores may be unpredictable. Extroverts have repeatedly been reported as doing well. Mr. Average remembers the many studies which confirm the findings on sheep and goats. There are also many ideas about readiness for inter-

personal contacts, adjusting to the roles in the experiment, and so on. Our experimenter knows all that—but will he do anything about it?

There are at least three things he could do:

1. He could select his subjects according to personality and attitude variables. Of course it would be nonsense for him to use maladjusted or neurotic subjects—deadwood in experimenting.

2. He could simply correlate the ESP scores with personality variables, or subdivide them into different categories. However, this would give little information on his real problem concerning the "dull" ESP cards, color cards, and emotionally-loaded pictures.

3. He could properly take the personality variables into account in evaluating his main problem by using appropriate methods; e.g., analysis of variance or covariance, etc. In other words, he could use personality or attitude test scores as tools for sorting out the real ESP effects from the deadwood in relation to his main hypothesis.

In order to obtain a "profile" of the experimenter, Mr. Average, I checked the 1959 and 1960 issues of the *Journal of Parapsychology* and the *Journals* of the A.S.P.R. and the S.P.R. In 76% of the reports, the experimenters did not select their subjects. In 21% the subjects could be said to be selected on the basis of previous ESP scores; e.g., in Forwald's work. In only one experiment (by Frances Greene) were variables other than previous performance used for selection. In fact, we have to ascribe these shocking habits to Mr. Average.

How about personality tests? Of the experiments reported, 59% used nothing of that sort; 20.5% employed only one personality test or attitude test, which is too little for sufficient information; 20.5% employed two such tests, which is more satisfactory. Tests well established in parapsychological use were applied in only six cases. There was not one experiment where such test scores were used as an adjunct to the evaluation of the main hypothesis.

We could conclude, then, that our average experimenter does not select subjects, but grabs them where he can get them. And if he uses personality or attitude tests, they are apt to be done for their own sake, not as aids in sifting out ESP from the data on

heterogeneous subject material ***in relation to the main hypothesis tested.*** Of course, our researcher knows he could do this, but as he is an *average* experimenter, there is no hope that he will. He will lull his bad conscience with a comfortable slogan: the experiments of Pratt and Osis with animals have suggested that ESP is present in several species. If it is present in such vastly different organisms as men, pigeons, and cats that eat pigeons, it should also cut through the little differences in human personalities. Naturally this is an easy way out of the dilemma, by what the psychiatrists call "rationalization."

Another problem might give our experimenter a headache, if he doesn't watch out: Is he taking into account what is relevant from general psychological knowledge (not only parapsychological) in his experiment? Suppose our friend's anxieties pop up in a dream one night. In this dream he may be faced by a grim old psychology professor who asks him questions like: "From what college class are you taking your subjects?" Now an art class was the only place he could get them. The Old Professor will wonder: "Do you think these lively artists will respond to emotional stimulation in the same way as the cooler temperaments from a mathematics class? Would art students respond to color the same way music majors do?"

In a cold sweat, our average parapsychologist realizes he has committed the major crime against scientific method—confounding experimental variables with subject variables.

But his nightmare is only the beginning. The Old Professor continues the quiz. "How do you know that your pictures are 'emotionally loaded'?" Mr. Average will try to explain how carefully he selected a sex scene, a threat of drowning, a lively skiing party, etc. But the grumpy Old Professor insists on objective evidence. There is none. Of course, sex stimuli would evoke very different reactions in a sexually satisfied person, in a sexually frustrated person, and in a person terribly afraid of sex. The subject whose favorite sport is skiing might be indifferent to the drowning scene and the swimmer unimpressed by the skiing picture. On the other hand, how does one know if this is the case? Should we ask the subjects themselves? But they will be completely subjective accounts. Then let us measure their galvanic skin reflex when they look at the pictures. Perhaps we should also ascertain their mani-

fest anxiety level with Taylor's scales. The worries just cave in on poor Mr. Average.

Then the grumpy voice becomes milder, as if setting a trap: "Do you think your extrasensory perception will be subliminal or supraliminal?" (Of course, according to most current writers it is a gigantic discovery in parapsychology that ESP is an unconscious process. Actually, it was termed "subliminal" sixty years ago by Frederic Myers.) He will continue: "Have you read up on the newest findings on subliminal perception; e.g., when Donald P. Spence demonstrates the 'fanning-out effect' of subliminal perception? The extrasensory perception of something emotionally charged might 'fan out' in obscure associations much more than if aimed at the unemotional designs of ESP cards. Have you thought about response methods which would eliminate this type of bias? Naturally you should also consider Benjamin W. White's ideas about the 'error matrix' in perception. According to his findings different error sources might be affected by your different stimuli. For example, the incoming 'signals' might be strengthened by emotional loading of stimuli, but also the 'noise' on the judgmental level of perception might be increased, preventing cue utilization."

Only now does our friend realize that he has not dealt with the psychological side of the experiment at all. The Old Professor gives him a very grim look.

But the dream goes on. The Old Professor disappears but a parapsychologist, Rhea White, sails in on a fine Jungian archetype: "Do you know what your subjects were doing the hour preceding the experiment?" she demands in the dream. "Do you know that the school-room experiments have indicated the importance of the subject matter of the class tested?" Horrible questions!

Then Winnifred Nielsen slips charmingly into Mr. Average's dream carrying the report on her experiment on moods and precognition under her arm. "Of course, the subject's physical well-being and mood are also important in your experiment. Are your subjects vigorous and fresh in the morning, exhausted in the evening, dripping with anxiety on exam days, or rejoicing about the Christmas holiday? Do you expect the same ESP score on all such occasions, or the same reaction to your emotional pictures?" More cold sweat is the only response our average experimenter can manage.

Margaret Anderson lands in one of the spaceships of her precognition experiments with Mrs. Gregory. She is all smiles: "How do you plan to manipulate your subjects, to get them in a proper emotional atmosphere, to relax them, to create a set for ESP, to impose proper motivation? Or do you really think they will score well without your doing anything for them?" What can one really do for them, if one plans to finish the whole experiment in half an hour? More distress.

Let us awaken our poor average experimenter now. I can imagine the cumulative effects of all that cold sweat. Please notice that most of the apparitions in his dream were female and tender-minded. But imagine how he would feel if the tough-minded Christopher Scott or Fraser Nicol were to materialize in his bedroom!

We now leave our colleague, Mr. Average, thoroughly convinced that psi in his experiment was treated as an island—and a very narrow island indeed. He feels shocked, but according to the textbooks, shock therapy is supposed to do a lot of good. Frankly, I was shocked, too, after reflecting on some experiments of my own. For example, I had been working on evaluations of a complex problem and punching my calculator buttons for hours. Then my secretary announced Mrs. So-and-So (my best subject) for an experimental session. I rushed into the experimental room and followed the same procedures as in previous sessions. The deviation dropped to zero. What was wrong? The subject was most co-operative. There were no external disturbances. And then it flashed through my mind that I could not possibly switch my own "set," my mood, my role in a minute from mathematical work to that of the social requirements of an inspiring experimenter.

The pioneer researchers got tremendous results, but then they really cared about them. The late Lydia Allison told me privately how carefully she used to prepare herself (note: not the subject, but herself) for experimental sessions with Mrs. Leonard. She would even go to that part of London where Mrs. Leonard lived, on the evening before the sitting. She would spend half a day preparing herself for the experiment, whereas I did not take even one minute. Mrs. Allison was wise indeed. And so were Hodgson, Myers, and Tyrrell.

We could discuss environmental variables as well, such as

Schmeidler's early experiments with the dark or sunny room, or Pratt's method of testing subjects at home. We could analyze other experimental situations such as psi experiments with hypnotized subjects. To my astonishment, a leading researcher on hypnosis and also a pioneer in parapsychology told me that no real ESP experiments with hypnotized subjects had been done. I mentioned a number of reports. He grinned: "If induction methods are not mentioned, depth of hypnosis neither described nor systematically varied, subjects' history in relation to hypnosis not given, would you call such business experiments?" "Of course, several hypnotists should be used with the same subjects," he added.

Again and again we observe that important variables have been forgotten, vital roots have been exposed to sun and wind. Is it any wonder that only a few of our plants manage to grow and that many others disappear forever in the cemeteries of gray files labeled "Non-repeatable Experiments"?

Let us take a hard look at some possible reasons why we hesitate to make adequate use of previous knowledge when designing our own experiments. Why are they frequently so unsystematic, spotty, amateurish?

There is available extensive information about subject variables, including personality, motivation, attitudes, and moods. We know a great deal about stimulus variables and something also concerning response variables, dating from the time of Frederic Myers. Biological and situational factors are also touched on by a number of researchers. And there is a tremendous field of psychological knowledge in general available to us. However, we seem to have a very strange attitude toward our treasures of knowledge. Something like a double standard emerges. Whenever we make "the case for ESP" or demonstrate "progress in parapsychology" everything is nicely listed. But when we are called on to use that knowledge by basing our experimental designs on it, strong anxieties arise. Perhaps we suddenly feel that the findings are not reliable enough; that they are not yet solid enough bricks with which to make a foundation. Often we wind up with a completely defeatist statement: "We don't yet know the important variables related to ESP; therefore we cannot control them in our experiments." In my opinion this is not a reasonable reaction at all, but rather resembles an anxiety outburst.

We do know quite a bit about important variables that could be useful to us. True, our knowledge is still very fragmentary but this is no reason to discard it wholesale from consideration in experimental designs. Much psychological knowledge is also fragmentary, even sporadic. There is no such thing as knowing *all* the variables in psychology. However psychologists are not shy in using incomplete or even vague findings in designing new experiments. This insures the continuity so necessary for fruitful research. The best evidence for this healthy trend is the long reference lists of experimental papers.

It seems to me that there is a dangerous disproportion between our courage and optimism in beginning new ventures and in our use of available information from the past. We are boldly running after the newest drugs on the pharmacological market, we readily line up heavy equipment like IBM computers, EEG's, polygraphs, plethysmographs. But how about utilizing what Charles Stuart, or Betty Humphrey, or Whately Carington found out about ESP? Treasures of knowledge more distant in history are practically ignored, as far as Myers, Sidgwick, and Prince are concerned.

I checked the reference lists of the experimental articles of the last two years in the *Journal of Parapsychology* and the *Journals* of the A.S.P.R. and the S.P.R. I counted only parapsychological references. In more than half of the reports, references start in the 1950's (12 articles), three in the 1940's, four in the 1930's and four were earlier than the 1930's, all by authors of foreign origin. We should beware of starting our history at 1950. If we lose the threads of continuity in our field, we may lose more than we can afford. Courageous but rational risk-taking is as necessary with respect to using accumulated knowledge as in new methodological departures. Latest developments of multivariate designs in psychology make this possible. They enable us to accommodate past information from experiments with much more ease than was possible in the thirties. They are also better adapted to the great complexity of personality than the Procrustean one-variable designs.

I am not arguing against radical innovations in psychical research; I am just viewing the whole problem of ESP embedded in a context of the crossroads situation in parapsychology. Present-day research on psi is boiling with methodological rebellion accompanied

by a sudden upsurge of creativity. New, bold manipulation of stimulus, response, and subject variables are emerging, together with new assessment methods. This methodological upheaval, as we pointed out, is a necessary consequence of the historical situation and should be encouraged.

Let us also keep in mind the basic trend in history that times of pronounced radicalism engender the dangers of discontinuity, of onesidedness, of sacrificing what is essential. I personally feel that with careful vigilance we can avoid this noncreative and dangerous part of the crossroads situation.

A statistician, Malcolm Turner, told this organization very plainly two years ago that if we incorporated previously known information in the designs of our experiments, we would tremendously increase the yield of information. He felt this was an urgent demand from the statistical point of view. I hope we are also able to see that the psychology of psi demands the same approach even more. Whether we are able to express our thinking in the form of statistical models or not, effective use of a wide range of past experiences is imperative.

This analysis of embedded ESP is mainly aimed at one practical purpose: to advance more effective research strategies. Let us take a look at the steps already taken in this direction and then decide what more may be done.

A definite help for student experimenters and amateurs has been various writings on how to conduct psi experiments. These have ranged from a few recipes on mimeographed sheets to fairly extensive handbooks. These might even be useful as primers for experimenters of real scientific caliber, provided they are taken as a starting-point for the experimenter's own reading. However, authoritative cookbook-style recommendations have severe limitations in scientific application. Usually they are prepared by senior researchers and reflect the sheer subjective impressions as well as the wide knowledge of the writers. Impressions are bound to reflect the personality of the author. For example, if the author is known to be a charmer in public relations, you can be sure most of his recommendations will be concerned with charming the subject in the test situation. The suggested techniques might work well for the author, but why should they be applicable to other experimenters who have quite dissimilar personalities? It seems to me that a broad and thorough-going

survey of the existing knowledge is a necessary prerequisite for each individual experiment at the planning and piloting stage. It is as unavoidable as any of the basic requirements of the scientific method.

The Parapsychological Association could be of very effective help to experimenters in their efforts to create research designs where psi is not an island but is embedded in optimal arrangements of important variables. When a researcher has done the groundwork of careful reading on the problem and thorough thinking through of his experimental problems, it is time to consult the experts available in our membership list.

I personally have found this method useful: to compose a form letter about an experiment in the planning stage and mail it to, say, thirty members whose training and experience might bear upon my problems. When I have done this, most of my colleagues have been glad to respond and have offered important suggestions and criticisms. Sometimes the persons from whom I expected the least sparked out the most important corrections for my design.

The P.A. conventions as well as the conferences organized by Duke University, the Parapsychology Foundation, and others give very healthy opportunities for broadening experimental designs by discussion. "Brainstorming" in our convention is already encouraged by our Research Briefs and free time periods; but brainstorming by correspondence could also contribute to the maturation of psi research.

Another practical step might be necessary if we want to associate ESP with factors which we ourselves are not well trained to deal with; for instance, modern drugs, physiological measurement techniques, and advanced statistical evaluations. In such cases we might need consultations so extensive that they could reasonably be expected only on a paid basis. I have seen research designs tremendously improved after a thorough going-over by top-notch scientists who received fees. Paid consultations are customary in psychology and there is no reason why we should be wary of them. So far as my own experience goes, scientists not committed to the psi hypothesis, but on the other hand expert in an area where guidance is needed, render much more valuable service than those accepting psi as a reality but having only mediocre knowledge of the problems in question. To save money on consultations might be in the long run

to squander it on poor experimentation. Furthermore, the tendency toward inbred methods and ideas in isolated individuals or seclusive groups can be lessened by exposing ourselves to consultants outside our group.

Parapsychologists since Myers' time have been aware of the concept of ESP integrated in the large and complex wholes of individuals, with interpersonal fields between them, engulfed by environmental and cultural factors. I have tried to illustrate with the dream of our experimenter, Mr. Average, that this integration of ESP in given situations could be at least partly sustained in our experimentation as well. Success in experiments on such elusive phenomena as psi depends heavily on both sophistication of design and method, on the one hand, and the art of handling subjects, on the other. If we can develop the more objective aspects of the scientific approach, there will be less need for reliance on the highly subjective art of subject manipulation. This might be one of the several possible means for exorcising the ghost of non-repeatability from the vast graveyards of unsuccessful parapsychological experiments.

References

1. Mangan, Gordon L. *A Review of Published Research on the Relationship of Some Personality Variables to ESP Scoring Level.* Parapsychological Monographs No. 1. New York: Parapsychology Foundation, Inc., 1958.
2. Murphy, G. Progress in parapsychology. *J. Parapsychol.*, 1958, 22, 229-36.
3. ———. Psychical research and personality. *J. Amer. Soc. psych. Res.*, 1950, 44, 3-20.
4. Myers, F. W. H. *Human Personality and its Survival of Bodily Death.* (2 vols.) New York: Longmans, Green, and Co., 1903.
5. Rhine, J. B. *New World of the Mind.* New York: Wm. Sloane Associates, 1953.
6. Schmeidler, G. R., and McConnell, R. A. *ESP and Personality Patterns.* New Haven: Yale University Press, 1958.
7. Thouless, R. H. Where does parapsychology go next? *Newsletter of the Parapsychology Foundation, Inc.*, Vol. 6, No. 5 (Sept.-Oct.), 1959.

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NEW FRONTIERS OF THE BRAIN[1]

By Michael Scriven

The banquet speech provides a suitable occasion for a survey of the whole or some part of our field. I shall use it to argue that this year sees parapsychology facing a serious crisis, a double crisis in fact, although this is not, I believe, apparent to everyone within the field. I intend to explain my grounds for my belief that such a crisis is on us, to indicate one way in which we can better prepare ourselves to meet it, and to provide some of the material necessary for a defense of the kind I think necessary. It is with some care that I do not say parapsychology is *again* facing a severe crisis: of its kind this is either the first, or at least much the most serious crisis the subject has faced.

Of course, parapsychology has often faced severe financial crises and in this respect it is perhaps now better off than in most preceding years—but it is not of these that I am speaking. A cynic might indeed say that we are about due to face the first of the crises that affluence brings, the wealth that has brought such teething trouble to the social sciences in this country. But whatever the economic interpretation, the crisis itself is an intellectual one. The evidential foundations of our subject, or at least the major branches of it, are severely threatened, in a way they have not previously been threatened, not by Spencer Brown nor by the exposure of medium after medium, nor by the devastation of Hettinger's work, or the elimination of the claims to seriousness of the healer-diagnostician de la Warr, the telepathic horse Clever Hans, the cloud-buster, the retrocognitive story of *An Adventure,* the ghost of Borley Rectory, and possibly the claims of the water and metal-diviners—or by any other of the similarly arduous, often brilliant and often unpopular pieces of scientific research that have frequently made me feel that we number in our midst the only competent and open-minded critics of parapsychological material, and feel proud that this was so.

The crisis that I now see upon us is not indeed due to any single

[1] An address delivered at the fourth annual meeting of the Parapsychological Association, Sept. 8, 1961.

issue or attack. It is due to the cumulative effect of two trends. One is what I shall call the evaporation tendency or trend, of which I have just given examples, the trend towards the reduction in the number of independent paranormal phenomena of whose existence there seems a substantial possibility. This negative trend is supported not only by examples of *disproof:* in evaluating it one must not forget the number of phenomena that have persistently *failed to recur* in the period of high-powered parapsychology, which I would take to be the last twenty-five years. Of these, ghosts are only the most famous; levitation, the rope trick, materialization, many of the old-time physical mediumistic phenomena, death by pointing the bone, many of the voodoo, mana, and witchcraft phenomena, and many others must be included. Honesty compels me to add that I would add to this list—despite the contrary opinion of those whose ability I greatly respect—the poltergeist. The field of the paranormal appears to have dwindled away under the scientific microscope of the parapsychologists. It is well worth noting again that evaporation has been achieved, not by the critics of parapsychology but by good scientific parapsychologists.

Apart from this negative trend—towards the evaporation of the paranormal supernatural—there is an almost equally significant trend in some areas towards its reduction to the normal. What we at first took to be paranormal phenomena turned out on closer examination to be only special cases of perfectly ordinary phenomena—and I do not mean that we *simply* became accustomed to something and thus extended the range of what we took to be normal. The classic example of this trend, which I shall call the absorption trend, is the fate of hypnosis which was originally a subject of special interest to our ancestors in the founding days of the Societies for Psychical Research. To begin with I believe this *was* merely a case of familiarity breeding content, but today I think we can see most, though not all, of the connections between the normal psychological phenomena of habituation, conditioning, and neural resonance, and genuine hypnotic effects. Notice incidentally the concomitant reduction in the field of accepted hypnotic phenomena, by comparison with the original accounts, beginning with the excessively skeptical early work of Clark Hull: thus we have here a combination of the evaporative and absorptive trends. Another

example from the field of suggestibility effects is the history of miraculous cures and the faith-healers. The careful analytical work of men like D. J. West has provided the destructive, evaporative beginning and we then see revealed only the commonplaces of psychotherapy which by absorption takes over our field. Again, the subject is far from closed even now, though apparently christening faith-healing "the placebo effect" has made it an entirely respectable subject for ordinary scientific inquiry! Closed or not, it is no longer part of the paranormal. The eventual disposition of the homing phenomenon in pigeons, etc. is not yet clear: if a natural account emerges, we shall be able to point once more to the contribution of the parapsychologists.

Perhaps the best examples of the absorptive trend, from my point of view, would be the fate of firewalking (where realism and physiology have taken over from exaggeration and parapsychology) and automatic writing, now a standard dissociative phenomenon.

Notice the way in which physiology figures in each of these absorptive examples. It is probably the development of this subject, and of neurophysiology in particular, which has been principally responsible for the narrowing of the field of parapsychology. This is one of the two reasons for the title of this paper. Think for a moment of Grey Walter's important Fourteenth Myers Memorial Lecture, which has just been published under the title "The Neurophysiological Aspects of Hallucinations and Illusory Experience." Grey Walter does not explicitly draw the following conclusions from his work, but they are plain enough. First, there are now excellent reasons for supposing that a very large number of people will occasionally have startlingly lifelike hallucinations (or dreams) whose subject matter is likely to be people or circumstances of great emotional importance to them. This seriously weakens one step in the usual inferences to the paranormal significance of a veridical experience. For we usually ask—in investigating alleged veridical phantasms—whether the subject *commonly* has startling experiences or dreams, our idea being that one "hit" among many "trials" is proportionately less significant. This question is not the only one we should ask, however, for there is also the possibility—now seen to be highly probable—that the frequency of such experiences may be very high *across* people even where it is unusual for any one person.

And if this "interperson frequency" is high, the fact that in one or two cases per 100,000 we get a correspondence to reality is just as insignificant as it would be if one person had 100,000 such experiences with one or two successes. In my judgment, the work of neurophysiologists, such as Grey Walter, has completely and finally undercut the possibility of drawing any conclusions about the existence *or* nature of telepathy and clairvoyance from spontaneous phenomena. Of course, the theoretical possibility remains open that some quantitative measure might be devised which would show a disproportion between the natural expectancy of success calculated from the normal base-rate of such experiences and the actual number of successes. But I believe all our work on evaluation supports me when I say that such a possibility is fantastically remote.

The second conclusion which follows from his work illustrates the absorptive trend in its most powerful form. I think we should probably now view our data on spontaneous phenomena as only data for the neurophysiologist and psychologist in their work on the mechanisms of projection and neural aberration and breakdown. Our conclusions undercut and our data appropriated!

This is a severe comment, and there is one perfectly rational reply which I myself thought adequate until recently. It might be said that the spontaneous phenomena could not be thought of as constituting the *grounds* of scientific belief in ESP, but they can serve to illustrate its mechanisms and suggest further laboratory experiments. Alas, even supposing we have unquestioned independent evidence for ESP the spontaneous phenomena could virtually never be reliably identified as instances of that rather than of coincidence plus neurological accident. Hence we could virtually never be sure we were identifying the mechanisms of ESP rather than those of normal psychology and physiology.

Nevertheless, one might respond to the whole tenor of my remarks so far along similar lines. One might say that whereas the over-all number of fields of parapsychology has shrunk under the scientific microscope, we have simply shed the dross and refined and expanded the yield from the fields of good ore, i.e., the field of laboratory ESP, so that actually there has been no over-all diminution in the field. This is indeed the only possible line of defense. The trouble with it lies simply in the vulnerability of the key work

here. By "key work" I mean the few experiments with overwhelmingly positive results. There are good reasons for assessing the evidential value of these as higher than that of many less striking experiments even when the combined mathematical probability from the latter is the same as from the key work. (See my "Modern Experiments in Telepathy" in *Philosophical Review,* April, 1956.) Now there has simply not been enough of these key experiments *in recent years* to stifle a feeling of uneasiness in many of us which has partly manifested itself in the search for a "repeatable" experiment. That feeling of uneasiness is now brought to a head by C. E. M. Hansel's attacks. It is easy to react negatively to the occasionally intemperate tone, to some of the definitely unjustified remarks or the extremely dubious philosophical framework of his remarks. It is exactly this kind of reaction we must studiously avoid, for it is exactly this kind of reaction to the work of Mesmer, Freud, and Rhine that has so often in the past identified the unscientific scientist. We must not make the mistakes that have often made our critics ridiculous. The question of importance is not "Do I like the man?" nor "Is he absolutely correct?" but "Can we learn something from him?" From Hansel I think we had better learn, because Hansel has propounded what I take to be the first really serious criticism of the key work. From George Price and David Spencer Brown—or, more accurately, from the discussions they precipitated—there was something to be learned, but it was of a general, philosophical kind, about the nature of evidence and randomness. Hansel has discovered *internal* evidence which lends some support to a counterhypothesis. He takes that counterhypothesis—I speak on the basis of his earlier articles and without having seen the manuscript of his book—to be fraud. It is clear that there are other possible ways of interpreting his data, perfectly consistent with the ESP hypothesis, but in any case he has exposed an Achilles heel in the data that we had not previously fully recognized. It is too highly dependent on too small a family of key successes. The effect of this is to make it too susceptible to being explained away by a single counterhypothesis, whether it involves fraud or not. At the stage when we realize this, we should become more seriously concerned by the relative rarity and antiquity of our key experiments. It is in my view now inescapable that we reverse, or at least partly suspend,

what was until now the sensible trend in parapsychological experimenting, the trend away from proving the existence of phenomena, towards proving something about their fine-structure. I am afraid we must go back to our primers, or at least arrange our work so as to include some work on them.

It is important to stress that the concern with repeatability is *not* the crucial matter. The Lisbon earthquake is not repeatable but its occurrence is extremely well established. *If* we can get repeatability, all the better; and eventually it is highly desirable. But it is not a requirement of all scientific claims that they be subject to test by repetition. It is only if they involve a claim with an unrestricted time variable, e.g., that Basil Shackleton is *always* able to score on (+1)GESP tests, or that most school children (i.e., now as well as in the past) display ESP rapport with their teachers. Our proper concern is with finding several subjects who—possibly under quite different conditions—do in fact maintain a score at around 30-50% above MCE as Mrs. Stewart did with her best agents.

The need for reconfirmation arises, not because repeatability is always a requirement, but for quite other reasons. The comparison we should have in mind is with the observation of the canals on Mars: the colossal difference between what is observable under exceptionally good and under normal seeing conditions was quite enough to account for the absence of confirmation for a long time, even with more powerful telescopes. But with the passage of time, we became increasingly aware of several kinds of counterhypothesis—for example, the optical illusions which lead an observer to "see" lines where in fact there are only dots. Today the feeling is one of general skepticism though not of complete disregard. I believe we must face the unwelcome fact that time and Hansel have undermined the key work in a way not unlike in kind, though importantly different in degree, from the way in which time and Dingwall have undercut our confidence in many of the best earlier séance reports in which we still cannot identify any specific weaknesses. This is not mere failure of nerve. It is a combination of two judgments—first, that even though the claims themselves *were* restricted in time, if phenomena of that kind ever occurred it is probable that *something like them* would occur somewhere else in some way. If it has

not, and if we have been looking hard, this makes it slightly less probable with every passing year that the original reports were correct. I do not say the possibility diminishes to zero, but it does, and should, diminish.

Second, there are good grounds for supposing that our critical faculties are steadily improving in this field, so that we eventually have grounds for suspecting that if *we* had been present in the earlier days, we might well have detected some loophole not apparent to the less sophisticated workers of, say, the last generation. Will these arguments, which evidently affect most of us in evaluating the early séance data, apply to Dr. Soal's much more recent work with Shackleton and Stewart? Hansel's suggestions make it clear that they do, because we can now see certain ways in which it would have been desirable to tighten up the control of his experiments. (I say nothing of Soal's latest work *The Mind Readers* because it is all too clear that a tremendous loophole exists in those experiments. However they do demonstrate exactly how *not* to run experiments with any new high-scoring subjects, i.e., do the experiments and then publish the design for open discussion.)

There is another respect in which we must learn from Hansel's attack, distasteful though it is to mention it. Whatever may be said about the manner in which he treated his hosts at Duke and Dr. Soal, there is no doubt that he was not always fully and freely accorded access to all the data in England at least, and there is no doubt that some of his perfectly valid criticisms of the records were "hushed-up" or held back instead of being given every kind of acknowledgment in the journals. The full facts of the various issues are not yet before us and I may have to change my conclusions about where the blame lies—but I shall not have to change them much because it is the absence of a proper discussion—early, open, and with replies all around—which is principally culpable, and that absence is quite indisputable. (In England public discussion has been essentially nil; here, recent and still incomplete.) A subject loses its claims to scientific respectability when it closes its publications to bitter or radical criticism; there are already too many examples of this behavior to be found around us, in the scientifically moribund field of psychoanalysis for example. Again, it seems a mistake to produce ad hominem arguments against critics who do

not employ them. Hansel's papers in the *Journal of Parapsychology* are models of temperance; the replies are not. Certainly it is Hansel who is alleging fraud and some indignation is understandable. It is neither necessary nor desirable.

Notice the difference between the kind of evaluation I am making and that of George Price. He argued from external grounds to the impossibility of ESP: I argue from internal grounds to the need for new foundations. One should not be alarmed by the inconsistency of ESP with current physics (even if that inconsistency existed, which I doubt); if one has valid experiments yielding odds of 10^{70} to 1 for ESP, then goodbye physics! But one *should* be alarmed the moment a shadow of doubt grows about the validity of those key experiments. It will be a test of the reader's detachment if he is surprised to find that I am myself in no doubt about the existence of ESP: and nothing that I have said above implies that I am or should be. Of course, it may be expected to set off the usual emotional reactions among those who think in terms of allegiances or antagonisms instead of logic, for it *is* critical. But I think it clear that Hansel's arguments, though significant in the way mentioned, are simply not as strong as his conclusions, nor as strong as those of Pratt and others in the discussion so far. (*Journal of Parapsychology,* June, 1961.)

This paper is an exercise in what one might call long-term logic, i.e., the structure of arguments involving long-range extrapolations, the conclusions being probability *modifications* rather than demonstrations or eliminations. So far it has been concerned with the conclusions that one might draw from noticing the shrinkage in the number of parapsychological phenomena, and I have suggested that this puts *added pressure* on the remaining fields to counteract the normal extrapolations of this trend. It could not possibly be grounds for outright rejection of perfectly good evidence. I have also suggested that these remaining fields are themselves suffering from certain internal extrapolations due to lack of recent key experiments. The added pressure from this further source unfortunately comes at the same time as discoveries which to some degree affect our confidence in some of the key experiments. I consider that this total situation constitutes a serious crisis and I have proposed certain measures to deal with it: a wider search for key subjects, a greater

willingness to face the deterioration of evidence with age, and increased analytical skills.

I now wish to turn to a closely related though somewhat more philosophical set of problems within parapsychology where it seems to me our defenses are very poor and in need of a concerted effort at repair. Anyone picking up recent issues of the *Journal of the S.P.R.* and the *Journal of Parapsychology* would be struck by the pervasiveness of debate or comment on the relations between the phenomena of physics and those of psychology and parapsychology. Interestingly enough, this has an extremely close bearing on the present evidential status of parapsychological phenomena. The service I hope to perform here is to set out some of the issues and the location on the battlefield of some of the forces, introduce you to a new force about which parapsychologists have not been fully informed despite the extensive discussion of it in journals, explain the relevance of the engagement to the problem of evidence and to certain general claims about the significance of parapsychological findings, and suggest my own conclusions.

On the major issue of the relation of the mind to the body and in particular to the brain, there are—as you know—two main schools of thought, the monists and the dualists. The monists, who believe there is only one fundamental substance or type of entity involved, may decide that it is mental (the idealists), physical (the physicalists—sometimes the term "materialist" is used synonymously with physical), or of some third kind (the "neutral" monists). In any case, it appears that they have no problem of explaining how the mind and brain affect each other; since there is only one thing involved, there cannot be a problem about any causal relation, which would involve two things. Their problem is to explain why there *seems* to be a distinction between mind and body, and a correlation or causal connection between them.

The dualists accept the existence of two fundamentally different kinds of entity, and are then faced with the problem of deciding how they are related. The parallelists maintain there is a constant correlation or synchronicity between mental and physical events, but not a causal connection between them, although within each separate sequence of events there may be causal connection; the epiphenomenalists believe that all mental events are caused by physical ones,

though not vice versa, and that, unlike physical events, no mental events cause other mental events; and the interactionists (as I shall interpret their position, which is sometimes ambiguous) believe that both mental and physical phenomena can cause phenomena of the same and of the other kind. I assume that "mechanism" means much the same as physicalism and materialism in contemporary philosophical parlance, but I shall try to avoid the term.

In our own field, we find Smythies as a monist, sub-species physicalist (he thinks sense-data are "end products of the causal chain of perception" and in the brain), with most of the rest being dualists; Zorab and Grey Walter being epiphenomenalists; Pratt, Rhine, Broad and Burt being—probably—interactionists. I am pleased to say that everyone seems to agree on two rather important propositions (1) that there is an external world, which rules out idealism, and (2) that some mental states are caused by some physical ones, e.g., headaches by excessive drinking, which rules out parallelism.

At this stage the disagreements start. In order to keep our feet on the ground I shall take four putative phenomena, two normal and two paranormal, and examine their implications for the mind-body problem, rather than talking at the theoretical level.

As the first phenomenon, consider the existence of a correlation between mental and physical states such that every variation in the former is accompanied by a variation in the latter (though not necessarily vice versa). This is a fundamental premise for physicalism and epiphenomenalism and is not usually denied by interactionists, hence its *truth* does not decide the dispute. But if it were *false,* only interactionism could still hold. So a great deal hangs on its truth in this sense. *Is* it true? We shall return to that question in a moment. I shall also comment on the exercise of the will to command the body, which R. H. Thouless and Cyril Burt take to be the everyday case of PK, and which the epiphenomenalists and physicalists take to be an illusion; on telepathy, which Rhine and Pratt take to disprove physicalism; and on survival which is thought by Broad and Zorab to disprove both physicalism and epiphenomenalism. Let us turn to the first putative phenomenon.

Is there a separate brain-state for every mental state? We have a quite negligible amount of direct evidence for this Hypothesis of

Neurophysiological Determinism, as it is sometimes called, but we certainly have no evidence to the contrary, and there is a reasonable though not overwhelming presumption in its favor stemming from the immense, though not unrestricted, success of determinism in general. It could apparently be disproved only if we could perform the physically virtually impossible experiment of observing the brain state to remain unchanged while the subject's thoughts changed. This is physically virtually impossible because the brain is never static and it is not only sustaining but also functional circuits that are in constant flux; moreover, the brain stem is almost inaccessible from complete observation, being masked by the outer layers.

Nevertheless, it appears conceivable that we might decide to abandon the Hypothesis of Neurophysiological Determinism (HND). Suppose, for example, that examination of the brain processes of individuals who come to a decision after meditation displays a condition during the meditation absolutely indistinguishable from the process during any other reflections, problem-solving, etc., terminating in a burst of activity in the centers appropriate for implementing the decision. In that case, interactionism would be in and the rest out. (I may add that it is in fact rather hard to reconcile interactionism with the HND, though interactionists rarely see this.)

Now, unless this unlikely and wholly nonparapsychological discovery is substantiated, the usual arguments in our journals will not suffice to defeat physicalism. Telepathy, for example, may be *thought* of as the influence of mind on mind, but then being in love is often thought of as a communion of souls and neither phenomenon has the least logical force against a carefully thought out position by those who are skeptical about minds or souls. For if physicalism is true, then telepathy is simply an example of a hitherto unknown inter-brain reaction which could be regarded as analogous to the discovery of magnetism. Of course, telepathy might be a useful accessory in an assault on the Hypothesis of Neurophysiological Determinism: a super telepath might be able to provide an independent check on whether a given subject *was* actually thinking when his electroencephalogram remained constant. But *only* through disproving that hypothesis could physicalism—materialism be disproved.

The same discouraging remarks must be made about the exercise of the will. If this is a case of mind affecting matter (the brain)

and if interactionism is true, then it is or it could be a model for the PK effect, but until those premises are established (by disproof of HND) it can be perfectly well accounted for by the physicalists and the epiphenomenalists. It is most undesirable for parapsychologists as scientists to commit themselves to a theory of extrasensory perception which requires the falsehood of the basic tenet of neurophysiological psychology, when all our phenomena can perfectly well be handled within a framework which is consistent with it. We should not fight at the new frontiers of the brain unless we have to.

Now surely there is *one* parapsychological phenomenon which has the most direct bearing on the mind-body problem. Broad, Thouless, and Ian Stevenson have recently argued that the IPA phenomenon, or survival, demonstrates the independence of the mind from the body. Broad indeed puts it by saying that this is the absolutely crucial and only parapsychological phenomenon which counts against monism and hence materialism. To this I would say, first, that it is then quite essential we apply to the evidence here the same "back-to-bedrock" attitude I have recommended with respect to the ESP work, noticing particularly how very long it is since we had good evidence and viewing the cross-correspondences as long overdue for critical reassessment. Secondly, I would say that we must not imagine we can show water vapor is fundamentally different from water by showing that it continues to exist when we remove the saucepan of water from which it originated. The mind may be a kind of field or emanation or related form of the brain which is eventually and rarely capable of independent temporary existence. This would not prove materialism to be false or dualism true in the supposed sense. I would myself think this kind of hypothesis inherently *more* likely than a radical two-substance dualism, partly because we have many analogs for it in nature and it would permit of explanation without the introduction of special correlation laws.

I am thus suggesting that we have often been too quick to think in a stereotyped way about the interpretation of parapsychological phenomena; carefully evaluated, they count less in favor of dualism than has been usually supposed among parapsychologists, if indeed they count in that direction at all. I view this as fortunate since I think we may expect an advance of the frontiers of the brain, of materialism if you will, which could well be of the greatest service

to us and should not be thought of as a threat to ESP in the way in which the advance of electromagnetic technology, exemplified by the snooperscope, was a threat to the physical mediums. There is no need to suppose that the identification of an ESP organ, or of an ESP mechanism perhaps analogous to known fields, would constitute either absorption or evaporation of our field. To discover that ESP is just as brain-dependent as abstract thought is not to discover it does not exist. Surely that point must be clear and we must not arouse opposition among neurophysiologists by behaving as if we were fundamentalists facing the development of evolutionary biologists. So far from being threatened by the subject, it can—and already has—afforded us new ideas and tools of great value. I think we have only a limited chance of survival as a science if we commit ourselves on what I see as purely dogmatic grounds to the denial of materialism. This would be the more tragic since I believe there are perfectly good logical grounds for rejecting monistic materialism. Moreover they apply to and overpower a much more sophisticated form of materialism than is usually discussed in parapsychological circles, a version which has replaced behaviorism as the philosophically "in" physicalist thesis. I add two cautions in case this sounds more exciting than it is: the weaknesses in this new version (known as the Identity Theory) are neither very novel nor in any way related to parapsychology. The plain facts of our own experience, logically analyzed, demonstrate the irreducibility of sensations to brain states. The development of the Identity Theory recently has been largely in the hands of Feigl and Smart and I shall not attempt to do more than bring it to your attention. Feigl's article "The Mental and the Physical" in *Minnesota Studies in the Philosophy of Science* (Volume II, ed. H. Feigl et al.) provides the most comprehensive exposition and a stupendous bibliography. (My own detailed comments will be found in the forthcoming Feigl Festschrift volume for Feigl, edited by P. K. Feyerabend.)

Dualism is true, but two questions remain. The obvious one: Which version of dualism? And an important but subtle one: How can science ever explain the fact dualism apparently must take as fundamental; viz., the existence of an enormous number of basic correlations of certain sensations, e.g., sharp shooting pain, with certain brain states? Cyril Burt sees this problem but answers it by saying these correlations would be inexplicable—just some more of

the basic postulated correlations in science. But I can think of only one or two such, compared with the *embarras des richesses* psychophysical dualism yields. This is contrary to the spirit of scientific economy and yet apparently inescapable. There is just one ray of hope here. The correlations are not between two independently identifiable properties, a fact recognized in the old "Perhaps when you see green, you see what I see when I see red" puzzle. I believe this ray of hope actually sheds enough light to get us out of the dualistic darkness and into the (dualistic) light. Similarly, I think we can demonstrate the truth of interactionism as an answer to the first question—which kind of dualism should we adopt. But I only *think* this: I have not yet found or constructed such a proof.

To conclude then, let me remind you of a development in the history of biology of great importance to us. For a long time there was a popular school of theoretical biology—it still has its supporters—the entelechists, which maintained that purposive behavior in organisms clearly demonstrated the falsehood of mechanism. This was simply a logical error, but it had serious scientific and social consequences for the entelechists. Scientifically, it misled them into fruitless theorizing about *élan vital* etc.; sociologically, it made them the reactionaries of the continuing scientific revolution in biology—they became the old men, the foes of progress. We cannot afford those consequences. At the very least we should recognize the primacy of the facts about ESP phenomena over any metaphysical framework commitments we may have, and their compatibility with several alternative frameworks. It is particularly tempting for the worker in an unpopular field to see the prevalent ideology of the conventional scientist as culpable for the unpopularity and the criticisms; and he may be supported in this view by the conventional scientist. Both are usually, and in the present case definitely, incorrect.

Thus I believe we face a double danger; over the evidence and over the interpretation of the evidence. If we do not exhibit a willingness to redo old work and rethink old thoughts I am afraid we shall find that the processes of evaporation and absorption will remove our subject matter.

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CALL FOR PAPERS FOR THE FIFTH ANNUAL CONVENTION

The fifth annual convention of the Parapsychological Association will be held at Duke University from Thursday, September 6, through Saturday, September 8, 1962. Any Member or Associate or any non-member sponsored by a Council Member may submit a paper for delivery at the convention. The time of presentation shall not exceed fifteen minutes. The paper must not have been published before the convention.

The intention to give a paper must be registered, together with the proposed topic, with the Program Committee Chairman, Dr. R. A. McConnell, Department of Biophysics, University of Pittsburgh, Pittsburgh 13, Pennsylvania, by April 1, 1962.

Papers may be presented in absentia. Such papers will be given by title and abstract only, with presentation time not to exceed five minutes. Should the author desire to do so, he may submit copies of his paper to the Program Chairman for distribution at the convention.

Abstracts of proposed papers must reach the Program Chairman by May 1, 1962, and shall be typed, double-spaced, in quadruplicate, on 8½ x 11 inch paper (or equivalent), on one side of the sheet only. The length shall not exceed 200 words. Abstracts of experimental studies shall include a quantitative description of the research. Papers may be either theoretical or empirical in nature, but preference will be given to the latter.

The abstract of a paper to be presented in absentia shall be accompanied by a statement from a Member or Associate expressing his willingness to present the paper at the convention.

Any Member or Associate may propose a symposium topic to the Program Chairman by April 1, provided he includes the names of the proposed participants and is willing to assume responsibility for the organization of the symposium.

Members or Associates may submit outlines of research in progress or soon to begin to Dr. Karlis Osis, c/o Parapsychology Foundation, 29 West 57 Street, New York 19, N. Y., not later

than one month before the convention. Such outlines may be presented at the convention under the "Research Briefs" section of the program. Delivery of a "brief" shall not exceed five minutes.

Decisions regarding the selection and arrangement of topics to be presented at the convention will be made by the Program Committee.

GLOSSARY

A description of the basic experimental methods, of the findings, and of the statistical procedures for evaluating ESP and PK results may be found in *Parapsychology, Frontier Science of the Mind* by J. B. Rhine and J. G. Pratt (published by Charles C. Thomas, Springfield, Illinois, U.S.A., Blackwell Scientific Publications, Ltd., Oxford, England, and The Ryerson Press, Toronto, Canada).

AGENT: The "sender" in tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

BT (Basic Technique): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

Chance Expectation = Mean Chance Expectation: The most likely score if only chance variation is present.

Chance Average: Mean chance expectation expresed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic cognition of the mental activities of another person.

CR (Critical Ratio): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearsons.)

CR_d (Critical Ratio of the Difference): The observed difference between the the average scores of two samples of data divided by the standard deviation of the difference.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

Backward Displacement: ESP responses to targets preceding the intended targets. Displacement to the targets one, two, three, etc. places preceding the intended target are designated as (−1), (−2), (−3), etc.

Forward Displacement: ESP responses to targets coming later than the intended targets. Displacement to the targets one, two, three, etc. places after the intended target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the pack before any are removed or checked.

ESP (Extrasensory Perception): Awareness of or response to an external event or influence not apprehended by sensory means.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, square, cross and waves (three parallel wavy lines).

ESP PACK: Twenty-five ESP cards.

Closed Pack: An ESP pack composed of five each of the five symbols.

Open Pack: An ESP pack made up of the ESP symbols selected in random order.

EXPECTATION: See *Chance.*

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects in employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

GESP (General Extrasensory Perception): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

MCE (Mean Chance Expectation): see *Chance.*

P (Probability): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARANORMAL: Parapsychical or parapsychological.

PARAPSYCHOLOGY: A division of psychology dealing with behavioral or personal effects which do not fall within the scope of physical principles.

PERCIPIENT: See *Subject.*

PK (Psychokinesis): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PLACEMENT TEST: A PK technique in which the aim of the subject is to try to influence falling objects to come to rest in a designated part of the throwing surface.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring responses to free material. A judge ranks the stimulus objects (usually pictures in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes which are non-physical in nature. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI-MISSING: Exercise of psi ability in a way that avoids the target the subject is attempting to hit.

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

PSYCHICAL RESEARCH: Older term used for parapsychology.

QD (Quarter Distribution): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RANDOM ORDER: An order of events which displays no trends or regularities that would allow any inference regarding one event from one or more of the others in the series.

RUN: A group of trials, usually the succesive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in any given unit of trials.
Total Score: Total of scores divided by number of runs.
Average Score: Total score divided by number of runs.

SD (Standard Deviation): Usually the theoretical root means square of the deviations. It is obtained from the formula $\sqrt{npq}$ in which n is the number of single trials, p the probability of success per trial, and q the probability of failure.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in parapsychology today is a probability value of .01 or less, or a deviation in either direction such that the CR is 2.58 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

SPONTANEOUS PSI EXPERIENCE: Natural, unplanned, occurrence of an event or experience that seems to involve parapsychical ability.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

STM (Screened Touch Matching): An ESP card test in which the subject indicates in each trial (by pointing to one of five key positions) what he thinks the top card is in the inverted pack held by the experimenter behind a screen. The card is then laid opposite that position.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP (including precognition) the objective or mental events the subject is attempting to respond to. In PK tests, the faces of the die which the subject attempts to bring up in the act of throwing. In placement tests, the designated part of the throwing surface.

Target Card: The card which the percipient is attempting to identify or otherwise indicate a knowledge of.

Target Pack: The pack of cards the order of which the subject is attempting to identify.

Target Face: The face on the die which the subject tries to turn up by PK.

TELEPATHY: Extrasensory perception of the mental activities of another person (as distinguished from clairvoyant cognition of objective events).

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.